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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE XVI.

Capillary circulation.—Substances which render the blood fluid.—Action of putrid matter.—Condition of the blood in scurvy.—Huxham's observations on the blood.—Experiments with acids.

GENTLEMEN:—The knowledge we have, up to the present day, obtained by the cultivation of animal chemistry is very limited; and, indeed, it is necessary to be deeply convinced of the importance of its study, and of the vast influence it is, in all probability, destined to exercise on the future advancement of our art, in order to contend with patience against the numberless difficulties of various kinds with which that study is beset. No branch of the science requires greater perseverance, practical skill, and devoted attachment to intellectual pursuits, on the part of those undertaking its investigation; while its cultivators expend their time in discovering a solitary fact, another outstrips them with ease by framing hasty hypotheses. But do not be discouraged from adopting the former course, from seeking laboriously for facts, by this seeming failure; you will merit better of your fellow men by the establishment of a single truth, than by the advancement of a host of brilliant speculations, though such may not be the opinion of the public, who judge blindly, for the most part, and seem to have a natural bent for the whimsical in science as in everything else; be assured, however, that it will be the opinion of those for whom such labours are really intended—the scientific few.

The number and variety of the materials entering into the composition of the blood render their separation a very difficult task, and the accurate appreciation of the properties of each a truly delicate one. Such being the case, the almost total impossibility of detecting a few atoms of foreign matter in that fluid is evident even *a priori*; yet the presence of such trifling particles is, in many cases, sufficient to alter its constitution thoroughly, to change its nature to such an extent that it becomes unfit to perform the functions for which it is destined. Consider the experiment of the injection of a few drops of putrid water into the veins: scarcely have they entered the circulation when the animal is seized with most serious symptoms; he ejects, by vomiting, a blackish, clammy liquid, which is nothing more than blood that had been exhaled on the inner surface of the stomach, and the mucous membrane is found studded throughout its entire extent with patches of blood effused into the subjacent cellular membrane. This is evidently not the result of a modification in the properties of the walls of the vessels; it is the consequence of the change of constitution undergone by the blood, of the loss on the part of that fluid of properties adapted to those of the ducts in which it flows. This we *know*, because we are ourselves, in the case supposed, the authors of the disorders we observe; but if the same symptoms had been developed without our interference, could we, unassisted by chemical analysis, have succeeded in making out the starting point of the disease? And this is precisely our state of ignorance as regards many affections.

If it were possible to remove from the blood, one by one, the various materials which, by their union and chemical combination, constitute it in the normal state, I have no doubt we should succeed in obtaining very interesting information on the principal functions of the economy. Properties which now appear insignificant to us, and which we scarcely mention in our descriptions of the fluid, are possibly closely connected with the persistence of the normal state of the functions. As a proof of this I may adduce the discoveries we made last session. When enumerating the physical characters of the blood, I simply mentioned its coagulability, without specifying parti-

cularly the influence that condition might have on the greater or less facility with which the hydrodynamic phenomena of the circulation are accomplished. At that time, indeed, I was possessed with a notion that the faculty inherent in that liquid of setting into a mass, must act as an obstacle to its progression in the vessels. But, Gentlemen, I then took an imperfect view of the question; a beautiful contrivance, of which I subsequently discovered the existence, is employed to counteract any such tendency. With the view of depriving the blood of the power of coagulating I removed its fibrine; the animal that underwent the operation died almost immediately after its performance; the sudden abstraction of the coagulable element of the blood invariably puts an end to life. Here, then (would the followers of a system of pure vitalism exclaim), here, then, instead of simplifying, you have complicated the problem of the circulation; you see into what a dilemma you have got by your attempt to apply the characteristics of inert matter to organised bodies. So long as the blood was fitted for circulation in the living tubes it was incapable of moving in metallic ones; you deprive it of the faculty of clotting, and it at once acquires that of moving in metallic tubes, while it loses the property of being fit to circulate in living ones,—physical and vital laws are here reciprocally exclusive.

This view of the matter, Gentlemen, seems to me not only to go further than the facts warrant, but also to be founded on a most unsound basis. By removing the fibrine we modified the composition of the liquid and not its vitality; if we reinject it into the circulation we find that it does not act as a poison; it is not the tissues it attacks, but the walls of the vessels; it is not because it touches the most delicate and susceptible parts of the frame that it causes death, but precisely because it is retained in its vessels, and traverses their coats by imbibition. So true is this, that you may remove a considerable quantity of fibrine without producing any evil consequence, provided you are cautious about abstracting a small portion only at a time, and allowing some time to elapse between each two operations. I have found that when the process is conducted in this manner, either no disorder of the functions follows, or else some very trifling disturbance; whereas symptoms of the most serious character supervened in animals suddenly deprived of all the coagulable material of their blood.

If it be true that the sanguineous effusions occurring during life depend solely on a want of adaptation of the properties of the liquid to those of its containing tubes, similar effects must be producible in the dead body. Of this you shall be yourselves the judges. Here is a piece of intestine which I have had injected with defibrinised blood; you perceive by the colour of the organ that transudation has taken place through the walls of the vessels: instead of well-defined vascular ramifications we find reddish streaks, the central part of which is formed of a membranous cylinder, and the circumference by tissues impregnated with colouring matter. Rupture would appear to have occurred in different spots, but this is a mere appearance; transudation has really taken place through the natural pores of the vascular tunics. Another fact proves more conclusively than the preceding ones, that it is not from losing one of its elements, the fibrine, that it becomes incapable of circulating, but from its having ceased to be coagulable; the fact is, that we have it in our power, by the injection of certain substances, such as the subcarbonate of soda, for example, to liquefy the blood, and produce lesions of exactly the same description as those determined by the abstraction of the fibrine.

Whoever has studied pathology, not in written volumes, but in the great book of Nature, must have been struck with one important fact, namely, that there exist certain morbid states which it is impossible to localise, and which, in reality, affect the entire organism. Certain diseases, I am aware, first attack a limited point of the body, and subsequently become general; thus, the lung may be first attacked; its tissue ceases gradually to be permeable, the blood is unable to find a passage through it, in order to come into contact with the oxygen of the atmosphere; the other functions then become affected. Prostration of strength supervenes, the tongue grows dry, the face assumes a vacant stare, the ideas wander, the limbs become the seat of involuntary trembling; hæmorrhage from the mucous membranes of the nares and intestinal canal follows, and the individual dies. Are we to ascribe all these disorders to the violence or malignity of the pulmonary inflammation? Certainly not; such would be a very erroneous view of the case; admitting the affection to have been primitively local, it must also be allowed that it was only from the moment the blood ceased to possess the characters of the arterial fluid that the entire economy commenced to suffer. When any other organ is similarly or even more seriously diseased, you will not observe the same series of phenomena; obstructions of the lung are the only disorders attended with such serious consequences as those adverted to; for the special function of that organ is to vivify the blood, and once it ceases to play its part that fluid becomes unfit for the support of its functions generally.

There are other circumstances wherein a change in the constitution of the blood appears to open the scene, and wherein the organic lesions are secondary phenomena.

The habitus of the disease is the same here, but the course of the symptoms is reversed, In the affection termed typhoid fever, the whole economy is affected at once; the different organs are, it is true, successively attacked, but *after* the whole frame has received the primary shock; thus, the intestinal follicles do not become red and swollen at the outset of the disease, but after the lapse of a certain time; at a later period still they ulcerate. They who look on the morbid state of those follicles as constituting the essence of the disease, reverse the natural order of things, and take for the cause that which is only an effect. Nothing is more common in these cases than obstruction of the pulmonary circulation; the vesicles become engorged, the bronchi choked up, and the patient dies asphyxiated. The spleen is the organ most closely resembling the lung in point of vascularity, and accordingly we find that almost all writers state that the spleen is constantly increased in size, diminished in consistence, and saturated with fluid, in subjects dying of typhoid fever. For my part, I would just as soon call that affection a *splenitis* as an *enteritis*: one and the other expression appears to me equally applicable, or, to speak more correctly, equally ridiculous.

The most striking point in diseases of this stamp, affecting the whole economy, is the fluidity of the blood, and the absence of the clot, or, if it exist at all, its small size and softened state. Up to the present time these conditions have been noticed by observers rather in order that they might appear to neglect nothing, than for the purpose of drawing from them any deductions capable of throwing light on the origin of the symptoms. I was long myself before I recognised the vastly important consequences of the loss of coagulability; now that my ideas on the subject are changed, my first care, when called in to a case, is to examine patiently the characters of the fibrine. If I find it soft, friable, soluble, and fusible, I am satisfied; I know thereby what organs must be affected, and I know what is the nature of their lesions.

One of the most remarkable among the morbid phenomena presented by animals into whose veins I had injected putrid water, was undoubtedly what is termed *inflammation* of the intestines; that is to say, exhalation of a matter having the colour of *washings of flesh*. This matter, which adheres to the mucous surface of the intestine, having there undergone a sort of gelatinous solidification, is nothing more than the fibrine of the blood coagulated in a peculiar manner. Of this I am perfectly certain, and for these reasons: I detached some of this intestinal exudation from the surface with which it was connected, carefully washed it, and thus separated the colouring matter from it, and caused the precipitation of delicate particles of fibrine. I employ the word particles designedly, in order to distinguish them from the globules, from which they materially differ; the globules, in fact, are, as it were, organised, affect a determinate form, and are all strongly similar to each other; what I term particles, are, on the contrary, an accumulation of very minute fractions of matter, of different forms and sizes, possessing no regularity or symmetry of shape. These little masses or filaments are not, therefore, composed of fibrine coagulated in the ordinary manner, inasmuch as the latter presents the cellular, vascular, and arborised organisation developed in the normal clot, and in the different adventitious formations.

I felt anxious to ascertain whether the *gelatinous* clots that I produce by mixing blood with certain substances, such as subcarbonate of soda, might not bear some analogy to the masses of fibrine deposited on the surface of the intestine in the cases to which I have been alluding. Certain it is that a very wide distinction is to be established between the normal coagulation of the fibrine, either in the cicatrices of wounds, or even the substance of the coagulum, and the aggregation, I would almost say the agglutination, of fibrinous particles accumulated on the surface of *inflamed* intestines. Some years past MM. Prévost and Dumas, who then believed that the globules were partly formed of fibrine, advanced, as their opinion, that these masses were in reality globules condensed as a product of excretion on the surface of the diseased bowel. It is now a matter of certainty that the globules and fibrine are two different things, easily separable from each other; but it is also certain that the authors just named caught a glimpse of the true condition of the question.

Another new and curious fact, which appears to me of very important character, is the influenced exercised, as we have lately discovered, by the pneumogastric nerve on the vitality of the blood. I, much to my surprise, observed that the section of the eighth pair deprived that fluid of its power of coagulating, but whether by a mediate or immediate action, I am unable to decide. Here is the blood of an animal who died from the effects of division of that nerve; although it has lain in this vessel several days it is perfectly liquid. The minute particles you see adhering to the sides of the vessel are assemblages of deformed globules, placed side by side, and very slightly adherent to each other; in a word, they are without any of the properties characteristic of fibrinous coagulation. This question is important; for if it be true that the nervous system exercises an influence on the coagulation of the fibrine, it is plain that that property must be henceforth looked on as a physiological one, and not as one capa-

ble of being influenced by the application of merely physical principles. This evidence in favour of the action of the nervous system on the coagulability of the blood, suggested to my mind an experiment, which I caused to be performed, with the following result. I had long since remarked that the arterial blood loses its scarlet hue in individuals labouring under an attack of well-marked apoplexy. In such cases the temporal artery is occasionally opened with the view of diminishing the constantly-increasing pressure exercised on the brain by the entry of fresh blood into its substance; I have myself frequently performed arteriotomy under such circumstances, and have found the blood black and viscous. I have not satisfied myself whether it coagulates like venous blood, but here is the experiment performed to elucidate the question:—I directed that a small opening should be made in the skull of an animal; that sufficient liquid should be injected through it, under the dura mater, to determine the characteristic phenomena of compression of the brain; and, finally, that the animal should then be bled from an artery. These three glasses are filled with his blood; in each, as you perceive, the serum is turbid; it holds some of the colouring matter in solution.

You will remark, also, that the clot is tremulous, undulating, and so far from being of the healthy firmness, that the simple weight of this glass rod is sufficient to break it in two. There is one point, however, which makes me doubt whether the experiment was made with all the desirable precision, namely, the colour of the blood; instead of having the black hue of venous blood, it is scarlet, not only on the surface, but throughout its entire mass. I will have the experiment repeated in my presence, in order to feel more satisfied as to the fact of strong compression of the encephalon being or not being capable of producing the effect on the blood to which I alluded.

You will recollect that when engaged last year with the question of the condition of the blood in scorbutic individuals, I told you that to its non-coagulability were ascribable the peculiar phenomena of their disease, such as swelling of the gums and the large petechiæ, and the yellow and black œdematous infiltrations more or less constantly found in the limbs and trunk. I have lately requested some blood of such patients to be sent me, for the purpose of repeating my last year's experiments. This repetition is far from being unnecessary, as you will presently see.

A short while past M. James inserted an essay in the "Gazette Médicale" on this subject, wherein he developed, with considerable talent, the ideas I broached respecting it last year, and supported the opinion that the alkaline character of the blood might be the cause of the non-coagulability of that fluid, as well as of the symptoms of scurvy. But, though this Essay is a meritorious production, it furnishes decisive proof how very necessary it is to draw general conclusions with deliberate caution, for the inference drawn in it is quite fallacious. Here is the blood of an individual affected with scurvy sent me by M. Leuret, physician to Bicêtre. The gums of the patient are, I am informed, considerably swollen; his teeth are quite loose; the surface is covered with large petechiæ, and some parts of the body are œdematous: nevertheless the blood presents a firm and consistent clot, the colouring matter alone is visibly altered in constitution, being of a brownish-red colour. Consequently we cannot look on this question as by any means settled.

Among the medicated drinks employed in fevers sulphuric water is one of those most frequently prescribed; I was consequently desirous of ascertaining the nature of its direct action on the blood. With this view I mixed a few drops of sulphuric acid with a larger quantity of water, proportionally, than that entering into the composition of the "sulphuric lemonade" of our hospitals, and injected a few centilitres of the liquid into the jugular vein of a dog. Death followed almost instantaneously, and the blood was found to be non-coagulable. At the present day no attention is paid to this condition of the blood, though our post-mortem examinations show it to be a frequent attendant on morbid conditions of the economy; but the medical men of the last century, whose powers of observation were quite as great as those of our own day, though their pretensions thereto were, perhaps, not so great, remarked the property of certain alcalious salts, as they called them, of preventing the blood from coagulating in glass vessels; and, further, noted the fact, that in certain epidemics that fluid lost the faculty of setting into a mass. I have of late been looking through the works of some of those writers. One among them, an "Essay on Fevers," by Huxham, a celebrated English practitioner, struck me particularly. The author, after having, in one of his chapters, established the differences between the thick and viscous blood of plethoric subjects, and that of individuals of an opposite temperament, in which the serosity abounds, and the globules are less numerous than usual, enumerates various substances capable of producing the last-named constitution of the blood, such as, among others, preparations of aloes, the volatile alkali, spirits of hartshorn, and laurel-water. Speaking of the latter, he says that "it makes the crassamentum vastly less dense, and exceedingly more soft and tender, and turns the serum red, or of the colour of Burgundy wine, as appears from the curious experiments of Dr. Nicholls and Dr. Laug-

rish."* And elsewhere he expresses himself in these terms:—

"But, besides these, there is, moreover, a third state of the blood, of more dangerous consequence than either; I mean a state of it that more immediately tends to *dissolution* and *putrefaction*. This is evidently the case in some scorbutics (as they are called) where, without any considerable antecedent sensible disorders (more than, perhaps, a kind of lassitude and languor), persons have, on a sudden, an eruption of violet-coloured, livid, or even black and blue spots all over their body, and forthwith fall into profuse, and sometimes dangerous and even fatal hæmorrhages, when they have scarce thought themselves, or been thought by others, to be under any manner of disorder. Abundance of instances of this kind happen: I have seen a great many, both in children and grown persons, and frequently foretold the ensuing hæmorrhage.

"Where women have such eruptions, or black or blue vibices, or large irregular spots like bruises, they are always subject to a vast overflow of the catamenia, if not to other profuse hæmorrhages. Nay, when persons of either sex are affected with these appearances, they are apt to bleed excessively from the slightest wound, and very often without any, from the gums, nose, guts, or urinary passages.

"The blood of such persons, when it hath been drawn off, in order to prevent the further progress of the hæmorrhage, as was imagined, (which, by the way, is very improper, unless there are very manifest signs of a plethora,) always appears a *mere gore* as it were, not separating into crassamentum and serum as usual, but remaining in an uniform, half-coagulated mass, generally of a livid or darker colour than usual, though sometimes it continues long very florid; but it always putrefies very soon."†

Further on he adds:—"Pestilential effluvia also soon destroy the crasis of the blood and produce an universal gangrenous disposition in the humours. This is evident from the frequent and fatal hæmorrhages, excessively fœtid sweats, vomitings and stools, and the general necrosis (*i.e.* gangrene) that follows, which have been observed in the plague and pestilential fevers by the best authors. The blood in these cases does not coagulate as usual."‡

He also speaks of an epidemic that occurred in 1737, in which pneumonia appeared frequently as a complication, and which was especially remarkable by the blood being so extremely liquid that it was difficult to arrest the hæmorrhage from leech-bites or opened veins. If therefore, Gentlemen, medical men who had advanced but little in the sciences of physiology, chemistry, and physics, were sagacious enough to make such important observations as those I have just quoted, we may reasonably expect to obtain results of the highest value from our researches in the same direction, now that the means of investigation within our reach are more multiplied and more sure.

I have entered upon a series of experiments for the purpose of ascertaining the effects of the mineral and vegetable acids on the blood, and have commenced them with sulphuric acid. With a view to discovering in what dose that agent becomes poisonous, I took eight glass vessels, each of them containing five centilitres of blood; to the first I added one drop of concentrated sulphuric acid; to the second, two, and so on. The first mixture is already almost completely liquefied, with the exception of a sort of sediment at the bottom of the vessel, perfectly distinct in nature from a true coagulum; the colouring matter is especially changed in constitution. Now, the conclusion deducible from this experiment, namely, that a single drop of sulphuric acid suffices to deprive five centilitres of blood of the power of coagulating, is certainly an unexpected one.

The alteration of the properties of the blood in the second vessel is much more marked; it has acquired a deep black colour, and there is a slight deposition of discoloured globules. In the third vessel, the black colour is still more evident, and the liquid is also more viscous; it is probable that some chemical action has taken place between the acid and fibrine, and I am persuaded the presence of such blood as this in the vessels of an animal would cause its instantaneous death. The appearance of the other mixtures is very much the same as that of the three we have examined; which seems to show that there is no great difference between the actions of two and eight drops of acid on a given quantity of blood: it would be well to experiment on smaller quantities, even, than a single drop, by dividing it, and studying the effect of the fractional parts. I have no doubt that a chemical action, the combination of the fibrine and sulphuric acid, must take place in these cases; but I have consulted some of our most distinguished chemists on the point without obtaining any important information from them, which is, indeed, little to be wondered at, when we consider that organic chemistry is in its infancy.

Some other inquiries of this kind have also led to results at variance with the opinions currently received respecting the effects of certain medicines. You are aware, for example, with what rapidity nitric acid coagulates albumen, and forms a nitrate with it. Well, I mixed ten drops of that acid with five centilitres of water, and poured them on two or three millilitres of

* Essay on Fevers, p. 45. Ed. 1769.
† Op. Cit. p. 41.
‡ Op. Cit. p. 51.

blood, and no coagulation has taken place; there is only a slight precipitate at the bottom of the vessel, but whether of fibrine or of albumen I am unable to say. In every trial I have hitherto made I have found all the acids act in very nearly the same manner on the blood: phosphoric acid, for example, which coagulates albumen with such energy, has completely liquefied the sample of blood I show you; a single drop only has been added, and yet the colouring matter has totally disappeared; there is an appearance of a precipitate of some sort, however, which I must examine under the microscope.

1*st Experiment.*—Citric acid.

This substance, which is used abundantly in effervescing and other draughts, has caused the liquefaction of the blood, though only employed in the same proportion as the nitric acid just spoken of; there is a slight precipitation of colouring matter. The injection of this agent into the veins of a living animal killed it instantaneously. We will examine its body at the end of the lecture.

2*nd and* 3*rd Experiments.*—Boric and arsenious acids.

These acids have also liquefied the blood, without producing any other remarkable result.

4*th Experiment.*—Pure tannin.

Among the substances that have particularly excited my curiosity stands pure tannin, recently discovered by M. Pelouze, and now employed as an astringent; I have myself prescribed it with a view to diminish the abundance of various secretions, hæmorrhages, and morbid discharges. It was supposed that by acting locally on the mucous membranes, by tanning them, as it were, this substance was enabled to put a stop to certain diarrhœas and abnormal fluxes. However, Gentlemen, the fiftieth part of a grain of this substance, which is, in reality, tannic acid, has prevented the coagulation of five centilitres of blood. It does not appear to have acted on the colouring matter, which you see here separated on a filter, and to all appearance possessed of its normal properties. According to the researches of M. Pelouze, tannin is a body possessed of the properties of an acid, and forming a tannate of fibrine when brought into contact with that principle.

5*th Experiment.*—Soluble cream of tartar.

Cream of tartar is administered in very large doses to individuals of every age and sex; mixed in the following proportion

Soluble cream of tartar .... 5 grammes,
Common water .....50 idem,
Blood ..... 2 centilitres,

it has given the whole a deep olive colour, and thrown down a slight precipitate of albumen. I do not mean to assert that this substance acts in the same manner, when ingested directly into the stomach; it is indispensably necessary to ascertain whether absorption through the intestinal canal does or does not modify the chemical properties of bodies; and if it does, how it modifies them. As for the action of sulphuric acid, it is reasonable to conjecture that, to use the figurative language of M. Dutrochet, that substance is an enemy to endosmosis; that is to say, that it is not imbibed by the tissues in the same manner as has been demonstrated to be the case with pure water.

6*th Experiment.*—Carbonic acid.

Here is the blood of an animal asphyxiated with carbonic acid gas; it is distinctly coagulated, and yet, as you will recollect, we have in other instances found the blood liquefied by this acid, particularly in the case of the female suffocated with the fumes of charcoal, whose case was communicated to me by M. James.

7*th Experiment.*—Tartarised antimony.

Tartar emetic, employed in the mixture I show you, has not prevented the blood from coagulating; it would, consequently, appear calculated to facilitate the circulation of the blood through the lung, and promote the resolution of pulmonary hepatisation; yet it causes, as I showed you in my last lecture, this very hepatisation.

8*th Experiment.*—Ioduret of potassium.

Iodine is among the substances that appear to possess the power of increasing the energy of coagulation; this mixture of a gramme of the ioduret and a centilitre of blood is completely solidified.

9*th Experiment.*—Hydrochlorate of baryta.

The hydrochlorate of baryta is employed in cases of engorgement, white swellings, &c.; it has here produced a peculiar effect on the colour of the liquid, which is arterial red; there is a tolerably firm clot too.

10*th Experiment.*—Borate of soda.

This substance has caused the formation of a clot with about the degree of consistence of currant jelly.

Here then are a certain number of substances which, far from opposing coagulation appear to promote it; they are more particularly common salt, hydrochlorate of baryta, tartar emetic, and ioduret of potassium.

I now proceed to the examination of the animal killed by an injection of citric acid into its veins. The first incision shows that the blood is liquid; its colour is very nearly the same as in the glass vessel, in which the proportion of acid employed was somewhat greater. According to my theory we should find a serious lesion in the pulmonary organs, and, in reality, the lung is not in a normal condition; blood and serosity both are effused into it, and the absence of

true engorgement depends on the fact of the injection not having been sufficiently concentrated to put a sudden stop to the circulation. Here, again, is a result which perfectly coincides with those already obtained: all of them will, I trust, have a beneficial influence on the ulterior progress of the science and practice of medicine.

EXTRACT FROM A LECTURE
ON THE
NERVOUS SYSTEM.

By MARSHALL HALL, *M.D. F.R.S., L. and E.*

JANUARY, 1839.

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LET me now call your attention to the true spinal, or excito-motory system. I shall, in the first instance, confine myself to a detail of *experiments;* and I think I may therefore assert that what I shall state may be considered as physiologically demonstrated.

If, the head of an animal being removed, you irritate the spinal marrow, those muscles which receive nerves from *below* the point so irritated, are *excited* into a state of contraction. If, instead of irritating the spinal marrow, you irritate a muscular nerve in its course, the muscle or muscles to which this nerve is distributed, are, in like manner, excited into contraction.

Haller, who specially treated of this subject, denominated the power in the nervous system, which is thus called into activity, the *vis nervosa*;* the power of contraction in the muscular fibre, he termed its irritability, or the *vis insita*. Of the former he observes, "Irritato nervo, convulsio in musculo oritur, qui ab eo nervo ramos habet. Irritato vero nervo, multis musculis communi, totive artui, omnes ii musculi convelluntur, qui ab eo nervo nervos habent, *sub* sede irritationis ortos. Denique medulla spinali irritata, omnes artus convelluntur, qui *infra* eam sedem nervos accipiunt; neque contra artus, qui *supra* sedem irritationis ponuntur." Haller concludes, "Conditio illa in nervo, quæ motum in musculis ciet, *desuper* advenit, sive a cerebro et medulla spinali, *deorsum*, versus extremos nervorum fines propagatur." And "Ut adpareat causam motus a trunco nervi in ramos, non a ramis in truncum venire."† It was a mistake, as M. Flourens has shown, to suppose that this power exists in the *cerebrum*.

This same power is more correctly denominated by Prof. Müller, the motorische kraft, or the *vis motoria*. This equally celebrated physiologist treats this subject still more at length, and has laid down the following laws in regard to the mode of action of the motor power:—

"1. The motor power acts *only* in the direction of the primitive nervous fibres going to muscles, or in the direction of the branches of the nerves; and *never backwards*.

"2. The mechanical or galvanic irritation of a part of a nervous trunk does not excite the motor power of the whole nerve, but only of the isolated part.

"3. A spinal nerve which passes into a plexus, and assists, with other spinal nerves, in the formation of a large nervous trunk, does not impart its motor power to the whole of that trunk, but only to the fibres which it affords in its course from that trunk to the branches.

"4. All nervous fibres act in an isolated manner from the trunk of a nerve to its ultimate branches."*

M. Flourens denominates this power the "*excitabilité*," and he has shown more distinctly than any preceding physiologist, that it exists in the whole of the medulla oblongata and medulla spinalis, inclusive of the tubercula quadrigemina, but exclusive of the cerebrum and cerebellum; and, of course, in the muscular nerves.

These statements convey a true view of the condition of our knowledge on this subject when I first began my researches. The *anatomical limits* and the *course of action* of this motor power were understood to be those which I have thus stated from these celebrated physiologists.

That this statement is true, in reference to the subject of my researches, I am enabled to prove, by detailing an unsuccessful experiment made by Professor Müller.

"I wished," says Professor Müller, "to ascertain whether the last of the spinal nerves, if they were divided at some distance from the spinal marrow, and galvanised, (their roots being still attached to this organ), would excite convulsive movements in the *anterior* parts, through the medium of the spinal marrow. The results were constant but unexpected.

"Neither the anterior nor the posterior roots occasion, when they alone are acted upon by galvanism, retrograde action into the anterior parts of the animal frame, as of the head. It seems, therefore," adds Professor Müller, "that the fibres of the

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* Some writers having been misled by this expression, to think there is a similarity in the views of Prochaska and my own, I must caution you that this vis nervosa of Haller is *not* the vis nervosa of Prochaska. I can scarcely tell you *what* the vis nervosa of Prochaska is, unless, indeed, it be *everything*. It is something which is *augmented* in *mania* and in *gout*.

† Elementa Physiologiæ, t. iv. p. 325.

* Handbuch der Physiologie, i. 656; and Dr. Baly's Translation, i. p. 680.

nerves do not communicate in the spinal marrow."*

Allow me now to call your attention to a series of experiments of my own. If, in a turtle, its head having been previously removed, we carefully lay bare the intercostal nerves, and pass across them the galvanic influence, we immediately induce movements in *all* the extremities, *the anterior* and the posterior. If we choose a nerve near the anterior extremities, these are most moved; if near the posterior, these, in their turn, are most affected. This experiment, as you will perceive shortly, forms the *basis* of the system of incident, excitor, motor, or excito-motory nerves.

That the source or principle of action in this experiment is identical with that already noticed, as acting in the spinal marrow and in the muscular nerves, is proved by an intermediate experiment. If, in a decapitated turtle, instead of laying bare the intercostal nerve, we denude the spinal marrow, and pass the galvanic influence across its substance, we also excite contractions in the *anterior* as well as posterior extremities.†

We have thus traced the influence of the "*vis nervosa*" of Haller, the "*vis motoria*" the "*excitabilité*," in a retrograde direction in the spinal marrow itself, and in an incident and retrograde direction in the intercostal nerves and the spinal marrow.

In this latter case we have, I think, a new kind of action, and, physiologically speaking, a new kind of nerve, that is, an *incident motor action*, and an *incident motor nerve*.

May I not affirm that this statement is the pure expression of *facts*, and destitute of *all hypothesis*? for I speak not of *fibres* or of *filaments*, about which there has been so much discussion; but of an obvious action in an obvious nerve. These two things are as "demonstrated" as any in physiology.

This incident motor nerve, thus *demonstrated*, is but one of a *system* of incident nerves, of which I must speak to you. But, before I proceed to that subject, allow me to detain you one moment, to say that what I have just stated is *not* to be found in the admirable works of *Prochaska*, or of *M. Flourens*, or of *any* physiologist with whose labours I am acquainted. Yet it is, as I have stated, the *basis* of that *system of nerves* and, I may add, of *functions*, to which I have alluded; it is that basis without which that system could not be satisfactorily established.

I have hitherto spoken of the *trunk* of one of the incident, excitor, or motor nerves. But I must now inform you that the extreme terminations or distributions of these nerves possess the excitor or motor, or, as I have ventured to express it, the excito-motory power, in a much higher degree than their trunk. If, having removed the head of a frog, you divide the integuments along the back, and raise them by means of the forceps, you will observe the trunks of many cutaneous nerves; now, if you irritate these trunks, no movements follow; but, if you irritate the cutaneous textures on which they are distributed, movements of a very energetic character are produced. (See my Memoirs, p. 48, § 21).

I now proceed with my detail of the series of experiments on the decapitated turtle. If you irritate the nostrils, or the palatine fringes, you excite, through the *trifacial nerve*, an act of inspiration. If you irritate the larynx by passing a probe along the trachea, you produce the same effect through the medium of the *pneumogastric* nerve. A similar phenomenon is produced by irritating the trunk of the pneumogastric, or the substance of the spinal marrow, near the points of their division respectively.

Have not these experiments proved, without one word of argument, the existence of *other two incident* nerves possessing *the special excito-motory property*? Do you not perceive the beginnings of *the system of incident excitor nerves* to which I have alluded?

Do you not further perceive that these nerves are not only excitors of muscular action, but, in the case last detailed, of *the act of inspiration*? And do you not now plainly see the application of the "*vis motoria*," the "*excitabilité*," and of the system of incident nerves, through the medium of which it acts, to *physiology*?

This *system* of nerves is further displayed in this diagram and table, and the *extensive class of functions*, for such it is, of which it is the *anatomy*, is displayed in this table.

Until the period of my researches, I believe that the pure motor power of the nervous system had never been applied to physiology,—that the idea of an *incident motor* nerve did not exist; and, consequently, that the system of such nerves, and the special physiology of this system, was totally unknown.

Why do I mention these things? Because it has been eagerly attempted to transfer the credit of what I have done to others, and especially to Prochaska, on the one hand, and to M. Flourens on the other. But I will ask you, whether the idea of an *incident motor* nerve exists in either of these authors? and, if not, whether the *system* of such nerves, with their *physiology*, *can* exist in them? In fact, Prochaska goes no further

* Handbuch der Physiologie, 1833, t. i., p. 632; Translation, p. 615.

† A similar experiment was performed both by M. Flourens and Professor Müller; (see Système Nerveux, 113; Handbuch, t. i., p. 632, omitted in the Trans.); but in these experiments the animal was not decapitated; the action of the special motor power could not therefore be distinguished from the influence of *sensation and volition*; and the experiment is, therefore, *not the same*.

than Whytt; he alludes to a *reflex* action, as seen in the *very obvious pathological* phenomena of sneezing, coughing, &c., and then all is confusion; for with these phenomena are associated, as of a *similar* character, the motion of the arm to the head, *said* to take place in apoplexy! the motion of the eye-lids when a person approaches your eye with a finger! the motion of the *heart!* of the *intestines!* &c. It is impossible to argue with persons of such confusion of ideas. And for M. Flourens, he does not make the slightest allusion to an *incident* motor action, or an *incident* motor nerve, or to a *reflex* action; and yet all the functions to which I now refer, *are* reflex, and affected by means of such a power and through the medium of such nerves! On the contrary, M. Flourens states a thousand times in his beautiful work, which is a model of physiological investigation, that respiration, for example, has its *primum mobile* in the medulla oblongata. It *has* its primum mobile in *incident excito-motory nerves.*

Now let me return to this table and diagram.

Anatomy of the True Spinal, or Excito motory System:

I. *The Incident, Excitor Branches.*
1. The trifacial, arising from
   1. The eyelashes,
   2. The alæ nasi.
   3. The nostril.
   4. The fauces.
   5. The face.
2. The pneumogastric, from
   1. The pharynx.
   2. The larynx.
   3. The bronchia.
   4. The cardia,—kidney and liver.
3. The posterior spinal, arising from
   1. The general surface.
   2. The glans penis and clitoridis.
   3. The anus.
   4. The cervix vesicæ.
   5. The cervix uteri.

II. *The True Medulla Oblongata and Medulla Spinalis, the Centre of the System.*

III. *The Reflex, Motor Branches.*
1. The trochlearis } Oculi.
2. The abducens }
3. The minor portion of the fifth.
4. The facial, distributed to
   1. The orbicularis.
   2. The levator alæ nasi.
5. The pneumogastric, or its accessory.
   1. The pharyngeal.
   2. The œsophageal.
   3. The laryngeal.
   4. The bronchial, &c.,
6. The myo-glossal.
7. The spinal, distributed to the
   1. Diaphragm, and to
   2. The intercostal and } muscles.
   3. The abdominal }
8. The sacral, distributed to
   1. The sphincters.
   2. The expulsors, ejaculators, the fallopian tubes, the uterus, &c.

All the nerves represented on this, the left of the table, are *incident motor nerves.* Some of them are *sentient*; but, whether sentient or not, they are demonstrably *motor*, and, whilst they are *motor*, they are *incident.* I beseech you not to allow these two words —these two ideas—to be disjoined in your mind; and there will then be an end of all dispute.

And now let me recall your attention to this table.

Physiology of the Reflex, Excito-motory System.

The action—1. Of the eyelids.
2. Of the orifices.
   1. The larynx;
   2. The pharynx.
3. Of the ingestion.
   1. Of food.
      1. In suction;
      2. In deglutition.
   2. Of air:
   3. Of semen.
4. Of exclusion. [gestion.
5. Of the expulsors, or of
   1. Of the fæces;
   2. Of the urine;
   3. Of the semen;
   4. Of the fœtus.
6. Of the sphincters.

It presents you with the *physiology* which corresponds to the *anatomy.* It presents you with an arrangement of the functions of *ingestion* and *egestion*, of the *orifices* of the *sphincters.* No one has pretended, except in the vaguest manner, and under the shelter of the phrase "*sympathetic actions*," that this extensive view of the subject had been taken before. Professor Müller distinctly states (Trans. p. 803) that the *reflex actions* have a limited place in *physiology.* Prochaska does not name one of these functions!! M. Flourens does not name one of them!!! And certainly no one has traced these functions to the *incident* action of the *vis motoria*, in *incident* and purely *motor* nerves, for the very existence of such nerves was unknown. In a word, gentlemen, it was impossible to explain the anatomy or the physiology of *deglutition*, of *inspiration*, of the various *expulsions*, until I published my discoveries of the true spinal marrow, and the excito-motory system of nerves.

What shall I say of the *pathology* of this system? It was impossible that such facts as teething and tetanus, facts as *obvious* as those already mentioned, should have been otherwise than associated with the nerves. How could *the whole class of spasmodic disease,—centric, centripetal*, and *centrifugal*, (to

use words not my own), be traced to, and associated with, a part of the nervous system still unknown? One of the most beautiful facts which I shall have to explain to you, is that of the very parts or organs enumerated in the physiology, being precisely those involved in the pathology; so that you have, in many instances, but to recall the former, in order that you may recollect the latter, and in this manner you will be greatly assisted in remembering the *symptoms* of the class of spasmodic diseases.

In conclusion, I must observe that the true spinal or excito-motory system, being the system of ingestion and expulsion, of the orifices and sphincters, is the system of actions on which depend

1. *The preservation of the individual,* and*
2. *The continuation of the species.*

It is in this large and extensive sense that the excito-motory system must be viewed, if we would see its real magnitude and importance. It is in this sense that it will be viewed by the anatomists and physiologists of a future age.

* * * * *

* I must again guard my reader against supposing that I use the first of these phrases in the sense in which Prochaska has used a similar phrase. Prochaska adduces, as an example of what he means, the acts of sneezing, cough, vomiting, which certainly tend to *remove* what would be injurious, and may be supposed to effect "nostri conservatio."

DECREASE OF THE INFLUENCE OF VACCINATION.

To the Editor of THE LANCET.

SIR:—Having perused, in the last Number of your Journal, your remarks upon the subject of small-pox occurring after vaccination, perhaps the accompanying table and remarks thereon, made from 90 cases which occurred under my observation during the last spring and summer months (while assisting Messrs. Major and Son, the surgeons of a district in the Hungerford Union), may be of some small service in elucidating facts on the subject at present under inquiry. I remain, obediently yours,

W. W. C. RANDOLPH.

Hungerford, Berks, Jan. 3, 1839.

The following table, considered abstractedly, certainly contradicts many opinions that have been brought forward on the subject of vaccination, and coincides with the opinion that vaccination has greatly diminished in its modifying effects upon the human system, since the time at which it was introduced, or since the time of the vaccine virus having been used in its primary state when taken from the cow, and which, with few exceptions, may be dated back to the time in which it was brought into use by its discoverer, Jenner.

It is denied by many that small-pox occurs after vaccination properly performed, and that those cases in which it does occur are to be attributed to improper vaccine virus being used, or the vesicles being broken before the virus has been absorbed into the system; there is no doubt that many cases occur from these causes, as also from another of which I have been witness, viz., that it is the custom amongst the poorer classes, in order to save the medical fee, in many instances to vaccinate each other, by which there is no doubt but that much impure virus is inserted into the system, in consequence of their not knowing at what time it ought to be taken from the vesicle, nor do they often think of allowing one vesicle to remain unmolested, and thus many persons supposed to be free from varioloid contagion, in reality are not so. These causes, however, would not account for the great increase that has latterly been observed of cases occurring after vaccination. In all

A Table, showing the different Forms of Small-Pox, in 90 Cases, distinguished according to Age.

Ages.	Cases before Vaccination.	Cases after Vaccination.	Cases of Small and Cow-Pox combined.	Deaths	
				Before Vac.	After Vac.
1—15.....	18	28	5	5	—
16—25.....	6	16	1	.....	1
26—35.....	.....	9	—	—	—
36—45.....	.....	5	—	—	—
50—70.....	.....	2	.....	1	—
TOTAL....	24	60	6	6	1

those cases in the above table occurring after vaccination, with very few exceptions, none could have occurred from any of the above causes, for most of them had been vaccinated by a practitioner who assured me he was most particular with all those that he ever vaccinated.

In looking at the above table the number of cases after vaccination seem to increase with the number of years that have elapsed since its introduction, and what better reason can be assigned for this increase, but that the virus diminishes in its modifying effects, according to the number of removes from the cow.

Some practitioners say, that to produce the modifying effects of the vaccine virus, it must be inserted into the arm in six places, whereas, when first introduced, it was inserted into two only. If it requires three times more the quantity of virus now to produce certain effects, than what it did 30 years ago, it is very evident it must have lost two-thirds of its modifying powers; this certainly argues in favour of the opinion that the vaccine virus has greatly diminished in its protecting influence, and that to produce these certain effects it must not be beyond a certain number of removes from the cow when introduced into the human system; and that when introduced it will exercise its protecting influence for many years, if not for the full period of the life of man; for many instances occurred to me in which parties who had been vaccinated for twenty and thirty years, although obliged to breathe in an atmosphere impregnated with varioloid contagion night and day, never experienced the slightest symptom of the disease. I also remarked that in many of the above cases occurring after vaccination, the premonitory symptoms and forms of eruption were equally as severe as those occurring before vaccination; indeed, the most severe case that occurred to me arose after vaccination, and which was the only one in which death occurred amongst the modified cases.

ACCOUNT OF A

MONSTROUS FŒTUS,

WITHOUT

HEAD, CHEST, AND ARMS.

By T. BOULTON, *Esq., Surgeon, Lambeth.*

ABOUT half-past nine o'clock on the morning of Sunday, the 23rd of December last, I was sent for to attend in labour Mrs. Wakefield, 38 years of age, residing in Arlington-street, Lambeth; she was pregnant for the fourth time, and in each of her previous labours had had living children. On my arrival I found the os uteri dilated to about the size of a half-crown, with the head of a child presenting naturally at the brim of the pelvis. After waiting some time I found the pains inefficient, and occurring at distant intervals, I therefore left, requesting to be sent for as soon as they became more powerful and frequent. I was again requested to attend at half-past five, P.M., when I found the head descended into the hollow of the sacrum, and uterine action being good, the patient was delivered in the course of an hour of a well-formed male child.

After the separation of the funis, on placing my hand over the region of the uterus, with the view of inducing its contraction previous to the extraction of the placenta, I found the abdomen still distended, and presenting that firm and tense character it does before delivery. This induced me to suppose it was a case of twins, and on making a vaginal examination I found what appeared to be the foot of another child, presenting high up; after the lapse of three-quarters of an hour, the uterus being indisposed to a recurrence of action, I was induced to administer, at intervals of ten minutes, three doses of the *dec. secal. cornut.*; this had the effect of renewing the pains, and causing a gradual descent of the foot, by which I was enabled to ascertain that some malformation existed. It was not, however, till after upwards of four hours that the feet had descended sufficiently low to enable me to make traction during the uterine efforts. I could now discover an immense mass above, possessing none of the usual outline. As soon as the feet were brought sufficiently external I applied a silk handkerchief round above the ankles, by which I was enabled to make greater extension, and after considerable difficulty succeeded in extracting an extraordinary formation, of which the following is a description.

I shall first state the external appearances, and afterwards those observed on an internal examination. It is a specimen of monstrosity, or arrest of development, possessed of neither head, chest, or arms. The formation commences about 2½ inches above the umbilicus, opposite what may be conceived to be the second lumbar vertebra. The superior part is somewhat rounded, and about three inches across; from this part it expands gradually on either side, to a little below the hips, across which the diameter is 7½ inches. Two inches from the top, in the median line, and between that and the umbilicus, is an elevation, about the size of a large walnut, but of a conical shape, on the pointed top of which there existed a vesicle, when first expelled as large as a pea, but which has since been ruptured, leaving a red point. This appears to be the termination of the canal of the spinal column, from which a considerable quantity of serous fluid oozed, resembling that usually found. This elevation has a tuft of

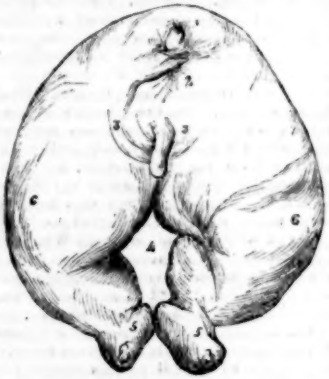
hair on it, which seems to indicate that Nature made some effort to form a head there. Half an inch below this projection is the entrance of the umbilical cord, which is of small size, possessed of two arteries and a vein; the umbilicus appears nearly of the natural form; three inches below it, and in the usual position, there exists a penis, somewhat large and elongated, without a glans, but with a thick prepuce; the inferior part is shelved off, and half an inch from its extremity commences the canal of the urethra, which is pervious only to the extent of an inch; the scrotum is of good size, but without testicles, and the anus is well formed. The mass across the hips is of large size, rounded, and possessing a gelatinous feel, and the thighs are very much expanded, measuring at the upper part between four and five inches across. The right leg and foot is that which descended first, the left being twisted, and lying across it, so as to present the sole of that foot upwards. There is an indentation of the skin at the part corresponding with the right knee. Immediately above the ankle the adipose substance swells out, giving the leg a deformed appearance. The foot is tolerably well formed; it possesses only a great toe, with a nail, and slight rudiments of the others. With regard to the left leg, it is considerably twisted, from the thigh downwards, and shorter than the right, lying across it as above stated. The formation of the knee is quite external to the limb, and more singular in its shape than that of the other. The foot has a less perfect great toe, but with longer rudiments of two others. On turning over this formation, so as to present the back part uppermost, the loins and sacrum have a general, rounded appearance. The nates are cleft, as in the natural state; the large size and width of the hips and thighs are more distinctly seen, as well as the curious shape of the knee on the outer part of the left limb. A ridge is formed along the inner part of that thigh, from the manner in which it lay across the other, and which it would be difficult to describe without an actual view of the parts. The right foot in this position also appears curved inwards and upwards, —the greatest length from above downwards is 11¼ inches.

With regard to internal appearances, on laying it open it presented at every part a very thick substance, covering the bones, &c., of a fatty and somewhat gelatinous appearance, with scarcely any trace of muscular structure; the only appearance of viscera was some rudiments of the bowels of a very small size. There was no liver, kidney, stomach, or pancreas. The penis was continued into a small elongated sac, intended for the bladder, but no ureters were attached to it; I, however, discovered the testicles in their usual position previous to descent, but very small. The space occupied by the bowels was very minute; the gelatinous mass was three inches thick on the thighs and 2½ over the abdomen. The umbilical arteries were continued as usual from the internal iliacs; there was no aorta; the vein could not well be traced. The general outline of the bones of the pelvis was tolerably well formed, but much contracted in size, and some rudiments of a spinal column existed, but without transverse or spinous processes. What appeared to be the spinal canal contained no nervous medulla, but two tolerably large nerves ran down on each side of it, giving off some branches to the pelvis and lower extremities. The bones of the lower extremity of the right side were much better formed than those of the left, the tibia of the latter being without cartilage at the knee-joint, and considerably separated from the articulating extremity of the condyles of the femur. On dividing the prominent mass on the anterior and upper part previously described, there were a considerable number of specks of nervous matter, united by cellular substance.

There existed only one placenta, of very moderate size. The two umbilical cords formed one continuous line across it, and appeared to have origin from the same part, so that I have no doubt there was a free anastomosis of the vessels of the two, by which the circulation in the imperfect mass was materially assisted; otherwise, without a fœtal heart, it would be difficult to account for the vigorous manner it must have been kept up for the nourishment of so large a mass, unless we allow, in some cases, an independent power of propulsion in the blood-vessels themselves. I am sorry I had not an opportunity of retaining the placenta with a view to injection. I observed no motion whatever in the mass after delivery, but it was evident the circulation had ceased but a very short time, as there was no appearance of decomposition.

A question of some interest suggests itself,—How far mental impressions on the mother, in the early periods of gestation, may have an effect in producing these malformations; and I perceive by a recent report of the proceedings of the London Medical Society, in your Journal, the subject has excited some discussion, and has its advocates *pro* and *con*. I believe the general feeling in the profession is opposed to the belief in such influence, but without entering into an argument on the point, we must admit, as is done by Dr. Blundell, in his lectures, that such strange coincidences sometimes occur that one feels a difficulty in laying aside the idea altogether. I would merely state in this case what was related to me by the mother herself a very short time after the birth of this monster. She informed me there was a boy residing

in her neighbourhood very much deformed in his upper and lower extremities. He is in the habit of trundling himself about on a little four-wheeled vehicle, in the manner most persons in London have observed other deformed objects, incapable of standing erect, and at other times drawing himself along the ground on the back of his hands and knees, his wrists being constantly in a state of flexion. On one occasion, about the second month of pregnancy, she was standing in a shop, ascended by three steps, when this creature dragged himself up them in the above manner; never having seen him before she looked with astonishment, but, as she states, not with terror or disgust; however, the impression fixed itself very tenaciously on her mind, and, to use her own expression, she "thought she should never get rid of it." It ought to be recollected, also, that all her previous children were well formed.



1, The conical elevation, with hairs attached.
2, Umbilicus, with cord ligature.
3, 3, Scrotum, with large penis.
4, Separation of the thighs.
5, 5, Feet, with rudiments of toes.
6, 6, Widest part, across the hips.

91, Westminster Bridge-road, Jan. 8, 1839.

ULCERATION OF THE ILIAC ARTERY

FROM A PLAGUE CARBUNCLE.

SUCCESSFUL LIGATURE OF THE EXTERNAL ILIAC ARTERY.

By JAMES LAIDLAW, *M.R.C.S. L. & E., and Surgeon to the European Hospital at Alexandria, in Egypt.*

JOHN HUNTER, aged 40 years, the commander of the English brig *Elliotts*, in the port of Alexandria, was attacked with plague on the evening of the 5th April, 1835, and was shortly afterwards removed on shore. He told me that he had lately been very much exposed to the weather, and had worked almost incessantly night and day in getting in a cargo of cotton for England, and he attributed the commencement of the malady to a cold caught by frequent exposure to the night air. His first symptoms were giddiness in the head, shivering, and great inclination to vomit, but not being alarmed, and thinking there was not much the matter with him, he merely put his feet in hot water, and took a dose of jalap and calomel, and went to bed, hoping to be better in the morning. During the night the fever greatly increased, and the following day he repeated the calomel and jalap, and soon afterwards took a dose of castor oil. Violent and continued purging then came on, and he says he found himself much relieved from the general oppression which he experienced at the commencement. Thirty-six hours after he first found himself sick he perceived a swelling in the right groin, painful to the touch, and attended with a sensation of stiffness down the limb. It then occurred to him, for the first time, that his disease was plague, and he ordered his seamen to carry him instantly on shore that he might place himself under my care.

When I first saw him he was sitting up in a chair, evidently very much exhausted, supported by three sailors. Notwithstanding a horrible and indescribable anxiety of countenance, he spoke to me cheerfully, and entered into a full account of the progress of the disorder, concluding with the strong hopes he entertained of recovering now he had got on shore. He then pointed out to me a black spot on the outside of the right knee, and a similar one on the back of the left hand, and which I immediately recognised as plague carbuncles; the one on the knee was about the size of a shilling, exactly resembling the eschar of a caustic issue, and surrounded with an inflammatory circle; the one on the back of the hand differed only from the other in being covered with a vesicle containing serous fluid. The bubo in the groin was nearly as large as an egg, very hard to the feel, and giving acute pain when pressed upon; it was as nearly as possible of the colour of healthy liver. I found the patient's tongue very foul, and covered with a thick coating of yellow fur; the pulse was pretty full at 80, intermitting at every fifth pulsation; the patient complained of great thirst, violent headach, and a restlessness that was quite insupportable; the skin was very hot and quite dry, a very slight perspiration appearing only on the forehead. I ordered him an emetic of ipecacuanha, and directed that a bolus of camphor and opium should be given to him after the action of the vomit. A poultice of linseed meal to be applied to the bubo and carbuncles, and to be renewed every four hours.

The next day I found him generally better

but getting very low; the sloughs of the carbuncles were increased in size, but a disposition to separation appeared to be commencing; the bubo remained stationary; the tongue had become cleaner, and there was some moisture upon the skin. I ordered him five grains of Dover's powder, with half a grain of calomel every four hours, to be washed down with camphor mixture, with a view of promoting diaphoresis. Towards evening he became much exhausted, and was greatly distressed by constant hiccough for several hours, until relieved by the administration of a large dose of laudanum with some sulphuric ether.

He went on much in the same way for four or five days, the exhaustion being combatted by opium and wine carefully administered from time to time, and at the end of the tenth day might be safely pronounced out of danger from the constitutional disease, but we had now other symptoms to attend to,—the carbuncle on the knee had ulcerated to the extent of three inches in diameter, and threatened to lay open the cavity of the joint, and had exposed many of the ligamentous fibres; the bubo in the groin, instead of suppurating, had for several days remained stationary, and a small slough now appeared, which gradually extended itself. At first the ulceration at the knee gave me great uneasiness, and produced considerable apprehension lest the knee-joint should become diseased, and the amputation of the limb rendered necessary; but it soon assumed a better appearance and granulated healthily. I anxiously supported the patient's strength with bark and acid, and a liberal allowance of wine, at the same time paying great attention to regulate the state of his bowels. Notwithstanding these precautions the slough in the groin increased daily in size, proceeding downwards in the thigh, till it was nearly as large as the hand.

It was not till the commencement of the month of May that any signs of separation appeared, but then the ulcerative action was set up with great vigour, and in a few days the slough came away, leaving a very large wound granulating at the edges, but sloughing at the bottom; the head of the pectineus muscle was exposed on the inner side, and the rest of the wound was composed of sloughing cellular membrane, extending upward under Poupart's ligament. The possibility of the artery being opened immediately occurred to me, and I cautioned the patient not to disturb the wound in the least, telling him, at the same time, that if any hæmorrhage occurred he must thrust into the wound a tent of sponge with a compress, which I prepared for the purpose. I gave similar instructions to his attendant (an English sailor), and impressed upon him the importance of acting with decision in case of an accident. For some days, however, circumstances looked more favourable; the granulations increased; the wound began to fill up from below, and only a very small piece of sloughy membrane remained, apparently running up under Poupart's ligament, and I, as well as the patient, began to think the danger nearly over.

On the 15th of May, while the patient was changing his shirt, a violent hæmorrhage broke out from the wound, the blood being thrown out with a jet to the distance of several feet; fortunately the patient was naked, and the compress at hand,—the English sailor, with the greatest presence of mind, thrust the tent into the wound, and held it there until I arrived. I found the patient fainting, but the bleeding was stopped after the loss of about two pints of blood. I applied a bandage firmly over the compress and directed the attendant to keep his hand constantly over it with some degree of pressure, and then dispatched a messenger for Dr. Estienne, a French practitioner in the town, with whom I was desirous of consulting upon the propriety of taking up the artery above Poupart's ligament. Upon the arrival of that gentleman he agreed with me in thinking that there could not be a doubt, from the quantity of blood lost in so short a time, and from the force with which it was thrown out, that it must proceed from the main artery, but that it was probable that only a small opening had been made in the artery, and that the compress might be found sufficient to form a clot and obliterate the artery. We, therefore, decided upon leaving the compress in the wound for forty-eight hours, then to remove it gradually, and if any hæmorrhage recurred, to tie the artery immediately.

On the 17th May we carefully removed the bandages and cut the compress away, bit by bit, but as a small portion immediately under the ligament seemed to be adhering by a clot, we thought it best to allow it to remain, to come away with the suppuration; the wound appeared healthy, and was dressed lightly, and a bandage applied. The patient was desired to remain perfectly quiet, and the assistant, provided with a compress in case of hæmorrhage, was ordered on no account to quit the bedside of the patient. In less than two hours we were again summoned to the patient, the messenger saying he was bleeding to death. I found that the bleeding had recurred about an hour after we quitted the patient, and the loss of blood before it could be arrested by the compress was much greater than at the previous hæmorrhage, and the patient had fainted; the bed and everything around him were saturated with blood, and had it not been for the coolness and admirable presence of mind of the assistant the patient must have perished. I determined upon performing the operation immediately, as I was satisfied that the recurrence of even a

very slight hæmorrhage would seal the fate of the patient.

Seated at the bedside of the patient I commenced the operation by an incision four inches in length through the integuments, as nearly over the artery as I could calculate, and by a second incision I cut through the tendinous expansion of the obliquus externus muscle, and then raising the muscular fibres of the remaining muscles with a pair of forceps I made a small incision so as to be able to insert the point of my finger. I then introduced a director, and, cutting upon it upwards and downwards, completed the external opening. I then found that a considerable quantity of blood had been forced into the cellular membrane surrounding the artery, and I had to clear it away with my fingers. I had no difficulty in passing my finger down at once to the artery, and with the flat head of a probe upon the fore-finger, and keeping it close to the artery, I worked a sufficient opening between it and the vein to admit of the passage of a ligature. The only instrument I was provided with for passing the ligature was an eyed probe, but I succeeded in passing it under the artery by bending it and drawing it out as a needle on the outer side of the artery. The ligature being thus left under the vessel I requested M. Estienne to pass his finger into the wound and satisfy himself that it was properly placed; as soon as he had done so I tied the ligature firmly, and bringing the two ends to the lower end of the wound, I brought the edges of the wound together with adhesive strapping, and applied a slight compress and bandage. I did not use any suture, as it was apparent an extensive suppuration must ensue in consequence of the cellular membrane being injected with extravasated blood, and some of the cellular membrane being already sloughy.

The patient being assured that he was now in perfect safety became more tranquil than he had been for some time, and, worn out by exhaustion and anxiety, fell into a sound sleep shortly after the operation, and on visiting him the next morning I found him in good spirits, the only inconvenience he experienced being excessive debility. The temperature of the limb was several degrees less than that of the other, and he said it felt as if it was dead. I directed that the limb should be rolled in flannel and friction applied from time to time.

The dressings were removed and the wound carefully examined on the second day, when it was found that a healthy suppuration had commenced, several portions of sloughy cellular membrane coming away with the dressings; the patient suffered no pain or inconvenience, and was in excellent spirits. As he had had no evacuation from the bowels since the first hæmorrhage, I ordered a common enema, but which returned without bringing away any fæcal matter. From this time everything went on well, great care being taken to support the strength of the patient by a liberal diet, &c. The wound suppurated healthily, but so profusely that I was under some apprehension for the artery, but from which I was relieved by the ligatures coming away with the dressings on the tenth day. The bowels remained costive, and as no evacuation had been obtained since the operation, I directed him to take ten grains of ext. colocynth. co. every four hours till the bowels should be acted upon. He took in this way forty grains of the extract, but no evacuation was obtained, and as the patient suffered neither from the constipation of the bowels nor from the stimulus of the medicine, I did not think proper to continue it. It was not till the fourteenth day after the operation that the bowels were acted upon, and he then again remained without an evacuation for six days, so that in the course of twenty-two days the patient had only two dejections. This torpidity of the bowels I attribute to the loss of nervous irritability consequent upon the great loss of blood and general exhaustion.

The wound made by the operation healed rapidly by granulation, and was perfectly closed a considerable time before the original wound which was the cause of the hæmorrhage. For a long period the patient was unable to use the limb, and notwithstanding every precaution a small slough formed upon the heel, but which did not prevent his moving about with the assistance of crutches.

The operation was performed on the evening of the 15th May, and the patient embarked for England on the 18th July in perfect health, but obliged to assist himself with a crutch in walking.

In laying the details of the above case before the profession, I am actuated by no idea that I am offering anything of an extraordinary or very novel nature. The operation has been now so often and successfully performed that it has almost ceased to be an object of curiosity; nevertheless, it is one which does not often fall to the lot of the surgeon to perform, and under the unusual circumstances of the artery being ulcerated by a plague bubo, and the difficulties of getting proper assistance during the raging of the epidemic, the patient escaping from the plague, nearly perishing afterwards by the hæmorrhage, and ultimately recovering from the operation, are circumstances which I trust, may render the case in some degree interesting to the reader.

Alexandria, April 26, 1838.

ON RABIES IN THE DOG,
AND THE
ABSENCE OF HYDROPHOBIA
IN THAT DISEASE.

By M. BARRY, *Esq.*, *Surgeon*, *Twickenham.*

AMONG the communications which have from time to time appeared in THE LANCET on the subject of *hydrophobia*, I do not recollect having seen one bearing reference to the falsity of that term, as applied to the disease somewhat more correctly named *rabies* in the dog.* The retention of so gross a misnomer is fraught with evil, and not calculated to strengthen the scanty claims of medicine to be styled a science. Neither would those of our body whose position invested them with the power of rebaptising drugs at the font of chemical knowledge, be at all impairing the standard of their utility by substituting proper for improper names in medical nomenclature. There are on record authenticated cases of bitten parties, who had been led into a fatal insecurity by an assurance from their doctors that all remedial measures were unnecessary, because the offending dogs had drank fluids, or rather had manifested no horror of water. During the last summer and autumn instances occurred within my own observation of people who had been induced to foudle and physic their own dogs, which were decidedly rabid, and this they did, under a like erroneous impression. Three were severely bitten on the hands, while indulging this amiable, but very dangerous feeling of benevolence towards their animals. My first consideration was to do for my patients whatever my knowledge of the nature of the inoculation suggested, after which I proceeded to examine the bodies of two of the dogs which had been allowed *à la Français*, to die of the disease, not of the doctor. The following appearances presented on examination, and they are such as I believe are almost invariably found on similar occasions.

Papilla of the tongue prominent; tonsils enlarged and hard; epiglottis vascular, with thickened edges; bronchial membrane inflamed; lungs congested in one case, and sound in the other; stomachs much affected by inflammation: the one contained about three ounces of a greenish fluid, with straws and horse-dung; the other contained about eight ounces of a fluid resembling weak coffee, with straws and what appeared to be the green moss found upon walls in humid localities. These dogs had drunk fluids throughout the course of the disease.

Nearly similar appearances presented in another examination very satisfactorily made by a very intelligent veterinary surgeon in this neighbourhood, and at which he allowed me to be present. Soon after these occurrences a terrier dog of mine, remarkable for his sagacity, I might almost venture to call it, his power of ratiocination, became rabid. A departure from his usual habits, which up to the present period had been most orthodox, first arrested my attention. His humour was on this occasion so pugnacious, that he attacked fiercely dogs of every degree that came in his way, and without giving them the benefit of any admonitory bark. Before, he was careful not to offend dogs larger than himself, and never lacked gallantry. Now, he regarded neither size, nor sex; his eyes were unusually bright, with some degree of strabismus; his bark at night incessant, but indistinctly uttered in a husky voice; swallowed much horse-dung, and other things which offered; his manner was hurried; and he spent much of his time in biting a particular part of his back. These symptoms were with me more than suspicious, and after properly securing him, I gave directions that no person besides myself should approach him, explaining to the servants the reasons for this quarantine, which extended to three days, when he died without the intervention of medicine. His walk was unsteady on the first day, and he ate heartily of meat and vegetables, and drank a great deal of milk and water; countenance anxious; he was perfectly obedient, but seemed to have unusual difficulty in comprehending whatever he was ordered to do. On the second day appetite less, but thirst increased; paralysis of hinder extremities; eyes sunken; snapped at imaginary objects, and showed much dread of his own shadow which preceded him when ordered into his kennel; bowels acted properly the first day, but on the second scantily. On the third day he lay crouched within his kennel; his countenance sad in the extreme; refused food, but always ready to drink, which, however, he could only imperfectly do, as the greater portion of the water escaped from his mouth, in consequence, as it appeared to me, of the paralysed and pendulous under jaw; deglutition was rendered almost impracticable, either from that cause, or it may have been by reason of his enlarged tonsils, or general exhaustion; but there was not the slightest appearance of fear of water, neither was any spasm induced by the attempt to swallow fluids. On the night of the third day he died.

This is an honest report of the progress and termination of the disease, as I watched it on this particular occasion.

Within the last few days a strange lurcher dog, in a rabid state, passed through this place, and is reported to have bitten eight other dogs, and I consider it a matter of great moment that the public mind should

* Our correspondent will find the disease, in some volumes of THE LANCET, indexed under the term Lyssa.—ED. L.

be disabused of the false and pernicious notion, by which they regard only those dogs as rabid which cannot drink fluids. The error can be best remedied by medical men, and if there be sceptics among them, from not having had an opportunity of observing the fact for themselves, I trust their incredulity will vanish before the testimony of the two most useful animal pathologists—Youatt and Blaine, the latter of whom says "that during more than twenty years experience of rabies in its most frightful visitations, he did not meet with a single instance where anything like a dread of water was shown, or any where spasm followed the attempts to take it." Mr. Youatt pointedly remarks, —"there is no dread of water; no spasm attending the attempt to swallow; but a most unquenchable thirst;" and John Hunter adds, in his remarks on the subject, "that mad dogs can swallow solids and liquids through the whole disease."

Those who regard the capability of drinking water as an infallible test of a dog's freedom from rabies will, by a parity of reasoning, have no difficulty in pronouncing that animal to be mad which refuses to drink during a temporary indisposition. Many valuable dogs are thus destroyed. For example, some years ago I was requested to see a dog of much beauty and value, supposed to have been bitten by one in a rabid state, and consequently doomed to be shot, because, from some cause or another, such as the absence of thirst or painful deglutition, he refused water while suffering from some trifling illness. There was not a single well-marked symptom of rabies present, and I pleaded for the postponement of the execution, but in vain, as the coachman had pronounced him *guilty*. I examined the body, and found nothing to militate against the opinion which I had given before. On the contrary, the absence of any of the ingesta which are almost invariably seen in the stomachs of rabid dogs, left not a shadow of doubt on my mind, of the injustice and ignorance under which this poor animal had been made to suffer.

Richmond Bridge, Twickenham,
January 3rd, 1838.

DR. ELLIOTSON.

We stated in a late Number of The Lancet, on information which we deemed to be correct, that, on the day of Dr. Elliotson's resignation, he (Dr. E.) had a dinner-party of students at his house in Conduit-street. We have since found that this statement was incorrect, no dinner-party having been given by the Doctor on that day.

THE LANCET.

London, Saturday, January 19, 1839.

"It is a pleasant thing to see brethren "dwelling together in unity: it is like the "oil upon Aaron's beard." Without pledging ourselves to the literal accuracy of the quotation, or to the justness of this simile, we undertake to say that it is very pleasant to see the medical men of a neighbourhood living together amicably, cultivating science peacefully, and labouring zealously for the public good. If we have sometimes found ourselves in the midst of war, and sometimes led the van, it is not because we have ever been insensible to the charms of the smooth, easy, oily state of things, so congenial to a learned profession, but because general practitioners and the great body of our brethren have had presented to them, ever since this Journal existed, the alternative either of agitation, or of tranquillity with dishonour, degradation, and exclusion from all offices of distinction and profit in the medical corporations. Under such circumstances war was indispensable. This war of principles is, however, a very different thing to personal squabbles, paltry contentions, mean detraction of each other—to the offspring of envy, hatred, malice, and all uncharitableness—by which the half-dozen practitioners of a small circle contrived formerly (we hope that it is rarely the case now) to run thorns into each other's sides, and render themselves ridiculous in the eyes of the community.

A few weeks since we noticed the public quarrel of the medical officers of the Salop Infirmary. We took our reports from a newspaper, and had to exhibit them in an unseemly, indecorous, not to say ludicrous light. We had Dr. James Proud Johnson, who is *President of the Shropshire Medical Association*, pointing out some imperfections in the by-laws of the Infirmary, denouncing certain adulterations of drugs, and moving a vote of censure upon a retiring officer,—we

had, we say, the excellent, but high-blooded Doctor upon the floor of the board-room, declaiming, and talking himself out of breath, upon all these and other subjects, in the midst of questions, answers, hisses, and interruptions, thrown in upon him by "the nobility and gentry," in volleys from all quarters,—yet stripped, and conducting himself valiantly, kicked in all directions (we speak metaphorically), and freely giving what he freely received; not much unlike honest Captain TRUCK, perspiring when mistaken for an English lion, and beset by the literati of New York.

We find from a report in the "Shrewsbury News," that the Trustees of the Salop Infirmary assembled last week to take into consideration the following notices of motion, which originated in the stormy discussion of the 6th of November last:—

Dr. J. P. JOHNSON gave notice that at the next general half-yearly board he should move that the 94th rule be rescinded, and that the following statute be adopted in lieu thereof:—

"That the Weekly Board shall purchase from wholesale London druggists all drugs that can be procured in a marketable state, which shall be prepared for use (as far as practicable) by the Dispenser, under the superintendence of the House-surgeon. All other drugs and chemicals required shall be obtained from wholesale London and provincial chemists, who can supply the very best articles on the most reasonable terms."

Mr. JAMES WHITNEY handed in a paper, of which the following is a copy, to be put upon the minutes of that day's (Nov. 6) proceedings, for next general meeting:—

"That in consequence of the injurious and unfounded aspersions thrown out by Dr. Proud Johnson this day, against the character of the highly estimable and respectable medical supporters of this institution, and of the express determination of those gentlemen not to act with him for the future, it is highly desirable for the interests of the Infirmary that Dr. Johnson should cease to be a physician to the institution."

The CHAIRMAN then read the following letter signed by Drs. Du Gard and Henry Johnson, and by Surgeons Burd and Dicken:—

"Salop Infirmary, Nov. 8, 1838.

"Gentlemen,—In consequence of Dr. James Proud Johnson having, on Tuesday last, at the half-yearly meeting of the Trustees, charged us with 'most disgraceful and unprofessional conduct,' we beg leave to state to the directors that we have fully considered the steps which ought to be taken under such circumstances, and we are decidedly of opinion that we cannot hereafter, either in consultation or any other business relative to the infirmary, act with him.

"With best wishes for the prosperity of this excellent institution, we beg to subscribe ourselves, &c. &c."

Doctor JOHNSON denied ever having employed the offensive expressions ascribed to him, and this was supported by the testimony of the reporters present. He declared that he had no intention of offering personal insult or professional disrespect to the medical officers of the institution, and the personal affair ended amicably. Dr. JOHNSON refused to bring forward the subject of drugs until the meeting of the Half-yearly Board, the time fixed in his notice of motion. No question is, perhaps, more important at the present moment than that of the purity or the adulteration of drugs employed in the public medical institutions, and we are sure that its importance will not be undervalued by the Trustees of the Salop Infirmary, when the matter is brought forward calmly upon its own merits, divested of all personal rancour and party spirit. It was not that the poor may be deluded and defrauded of health and life by the employment of inefficient remedies that the interests of the Salop Infirmary were advocated by the eloquent voice of the late venerable Archdeacon CORBETT; it was not for this that thousands of pounds were lately subscribed by the principal people in the county. Lord HILL, Mr. BATHER, Mr. WHITNEY, no trustee, and least of all the medical officers, can look with complacency upon the present arrangement,—the present law of monopoly, which gives two chemists in the town the *exclusive* right of supplying the Infirmary with drugs at their own prices, and leaves the institution at their mercy, by never having any of the remedies carefully examined, tested, or analysed by scientific chemists. In point of price we remarked before, that the market should be thrown open to competition. To say that drugs shall be obtained exclusively from London

would be as bad as declaring exclusively in favour of Messrs. BLUNT or Messrs. WHITNEY; for the latter are well known to have a very respectable knowledge of chemistry, and may be induced, by the consciousness that the Infirmary will take the best and cheapest samples, to manufacture better articles than can be supplied from any other quarter. There is, or was, a law in China that no man should be a governor or a judge in his own province, but it is unnecessary to extend this principle to the chemists and the infirmaries, as better guarantees are attainable.

We stated that a silly quack, calling himself a chemist, had been *untowardly* admitted as a pupil at the Salop Infirmary, without abandoning his trade or renouncing his extensive, perilous, and ignorant practice. It would seem, from the following highly flattering memorial, signed by seventy-five practitioners in Shropshire and North Wales, that in deprecating the introduction of this person into the hallowed precincts of the medical profession—in fact, in *roasting* this Mr. ONIONS—Dr. JOHNSON's conduct has, upon the whole, met with the approval of a majority of the most repectable of his brethren.

"We, the undersigned medical practitioners, feeling convinced that Dr. J. P. JOHNSON, in the part which he took in the proceedings of the last general half-yearly Board of Trustees of the Salop Infirmary, was actuated solely by motives of regard for the prosperity of the institution, and the honour and respectability of that profession of which he is a member, do hereby express our disapprobation of the attempt made to deprive the patients of the Infirmary of his valuable services.—Dated the 3rd day of January, 1839."

WESTMINSTER MEDICAL SOCIETY.

Saturday, January 5th, 1839.

ABSENCE OF MENSTRUATION IN A MOTHER.—OBSCURITY ATTENDING THE PRESENCE OF MALIGNANT DISEASES.

MR. HARRISON inquired if any member had known an instance in which the mother of a large family had never menstruated? He had known such a case.

Dr. JOHNSON had never seen an instance of the kind. He had, however, under his care at present some members of a family in which there were five daughters, the ages of whom ranged between twenty-six and thirteen, who, though all in excellent health, had never menstruated.

Mr. STREETER exhibited the œsophagus of a patient, of whom he related the following particulars:—He was fifty-nine years of age, and first came under Mr. Streeter's care in November last. He had for some time complained of difficulty of deglutition, and was now unable to swallow solids; the passage even of liquids gave him considerable pain. The neck was tender at one part, but there was no tumour; the countenance was altered, but did not bear an appearance characteristic of malignant disease. Expecting that there was ulceration, a simple œsophagus tube was passed to see if its presence could be borne, and if so, it was intended to apply the nitrate of silver in solution to the affected part. During the passage of the tube the patient complained of much pain, and it could not be passed beyond a certain distance, its further progress being obstructed by some strong resisting agent; on withdrawing the tube it was found filled for about two-thirds of an inch by a matter which appeared to be malignant. Treatment was of little avail; catarrhal symptoms set in, and on Monday last he died. On examining the œsophagus the disease had apparently commenced in the neighbouring glands, and thence extended to the alimentary canal. The œsophagus, which was disorganised to the extent of nearly two inches, was contracted to the size of a swan-quill; the mucous membrane was ulcerated over the whole of the strictured part; a small pouch was detected posteriorly, by which the end of the tube no doubt had been obstructed. The edges of the ulceration did not exhibit any cartilaginous or hardened bands.

Dr. ADDISON inquired if there were signs of any malignant disease in the belly, particularly in the liver, in the case related. Many of the depositions under the submucous membrane, supposed by some to be malignant, were considered by Andral and others to be simple hypertrophies of the submucous cellular tissue. The presence of glandular enlargement in the neighbourhood, was no evidence of the disease being malignant, as this enlargement would arise from any local irritation. Cases did occur, and by no means unfrequently, in which scirrhus of the pylorus, and stricture of the intestines, were accompanied by general contamination of the system; the disease went through its usual course, parts in the neighbourhood became affected, and they formed one mass of malignant disease. In these cases there was malignant deposition in other parts. In the majority of cases of stricture of these parts, however, no consti-

tutional effects were present; the only prominent symptom being a remarkable paleness of the countenance (not that indicative of malignant disease), and after a time great emaciation, as the result of imperfect nutrition. In these latter cases the disease was merely a local one, being, in fact, a species of hypertrophy.

Dr. JOHNSON related the case of a physician who for ten years had been a sufferer. For the first five years the most remarkable feature of his case was a constant restlessness, so that he did not sleep five minutes at any one time during that entire period; there was no pain, but a great fidgettiness, which kept him awake. He could not explain the reason of this; there appeared to him to be no cause for it. During this time he got remarkably fat. He was restless for the next five years also, but during the last two or three years of his life, in addition to this symptom, he had feelings which convinced him that he had disease about the sigmoid flexure of the colon or at the commencement of the rectum. For the last seven or eight months he became incapable of retaining any kind of food on his stomach. The disease in the lower bowel increased, and he suffered from great uneasiness. To these symptoms, for the last two months, jaundice was added, and the skin became the colour of a piece of dark mahogany. Pus and blood were discharged from the bowels, which were acted upon with the greatest difficulty. He was eventually starved to death, from incapacity to retain food, his body presenting a full fatty appearance, while his limbs were remarkably emaciated. *After death.*—The chest was not examined. There was a cancerous ulceration of the sigmoid flexure of the colon, just where it joins the rectum, of considerable extent, with thickened and hardened edges. Above this the calibre of the gut was so contracted that the little finger could scarcely be passed through it. The pancreas was one mass of indurated structure, containing calcareous matter, and completely blocking up the biliary ducts, so that there was not the slightest passage for the bile into the duodenum. The liver was granular. The gall-bladder contained a teaspoonful of a putty-like matter. The pylorus was scirrhous. Every one of the abdominal viscera was loaded with fat, the parietes being two inches in thickness. This was remarkable, considering that the supply of bile was entirely cut off, that the stomach would retain nothing, and that he had only been supported by injections for the last few months. This case was interesting as showing the extent to which malignant disease could proceed without destroying life.

Mr. STREETER, in answer to the question of Dr. Addison, stated that he had not been able to make a minute examination of the viscera in the case which he had related. He had no reason to suspect the presence of malignant disease in other parts. The liver was healthy. He believed that fungoid diseases might occur in almost any part, and yet be unaccompanied by disease in other organs. He recollected a case of fungus hæmatodes in the neighbourhood of the shoulder-joint, in which the disease was entirely local, indeed some practitioners who had seen the case, thought it one of exostosis. The case he had brought forward, he considered resembled that affection which is frequently found about the neck of the uterus and upper part of the vagina, in which all the tissues become converted into a gristly or scirrhous structure, and which disease is generally considered to be malignant. What was the proportion of cases in which there was simple stricture of the œsophagus to that in which the disease was similar to the one under discussion?

Mr. RODERICK had found, when disease of the œsophagus was situated high up, that the epiglottis became affected, and dyspnœa was induced; whereas, if the disease was situated at the lower end of the tube, vomiting was a prominent symptom.

Mr. HALE THOMSON said that malignant disease might go on for a long time in the body without its presence being suspected. An instance of this kind had occurred some time since in the Westminster Hospital. A man, between 60 and 70 years of age, apparently worn out by old age and want, was admitted for retention of urine; his water was drawn off; he had no symptom of disease; but he died in three days. After death every organ in the body was found to be affected with fungoid disease, very much resembling fungus hæmatodes; it had affected the brain, lungs, heart, stomach, liver, pancreas, spleen, and intestines; yet this man's history did not convey an idea that disease of any organ had been going on. Mr. Kiernan had examined the growth in a variety of the organs, and he had considered that it was a good specimen of fungus hæmatodes. The symptoms attending the presence of fungoid disease were, at present, so little understood, that the best practitioners were liable to error as to the result of tumours, respecting which they were consulted. Questions of great importance were connected with this subject. For instance, would malignant disease remain hidden in the system for a long time without producing any ill effect upon it? What were those circumstances which had the greatest effect in developing or accelerating such disease? Did it ever exist as a local affection merely? The following case might throw some light on the subject:—A policeman, in a quarrel, had his arm fractured. At the expiration of six weeks a small tumour was detected exactly in the seat of the fracture; he could use his arm well, but the tumour increased in size, and became so painful that he pre-

sented himself at the hospital for relief. The tumour was now as large as a turkey's egg, and very painful; it was pronounced to be fungoid, and probably fungus hæmatodes. Amputation was proposed, but rejected by the patient, though at this time a good stump could have been formed. When the disease was more advanced a consultation was held on the case, with seven or eight of the most distinguished surgeons of the day. The majority considered, that even if the tumour was entirely removed by amputation at the shoulder-joint, it would probably return, as the constitution, no doubt, was affected. Two of the surgeons thought the man might escape after the removal of the growth, but that it would be hazardous; two others thought he would recover. The man preferred to die rather than run the risk of a failure in any operative procedure. The disease spread, and increased in size, till it became as large as a man's head, implicating the shoulder-joint, and, at the end of eight months, having reached the cavity of the chest, proved fatal. After death the fungoid disease was found to proceed from the seat of the fracture, to which it was attached by a thin pedicle; there was not a trace of fungoid disease in any other part of the body. He (Mr. T.) had a firm conviction that if this man had suffered amputation, when only two inches of the humerus were free from disease, he would have done well. A few weeks since he had been consulted by a gentleman, in excellent health, with a tumour precisely similar, which had followed fracture of the arm. The tumour was at present small, and recollecting the case of the policeman, he had at once recommended amputation. The return of fungoid disease after removal he believed to be less common than generally stated. Two gentlemen he knew had lost a fungoid testicle each two years since, and there had been no return of the disease in any part. He would ask the members whether fungoid disease, resulting from recent fractures of bones, were connected frequently with fungoid disease in other parts. He believed that, generally speaking, they were merely local, and if removed early the operation would be successful.

Mr. Chowne remembered a case in which a fungoid tumour was removed from the foot, and had not returned at the end of eight years.

Dr. Brooke related the case of a woman who died of fungoid disease of the glands of the inferior maxilla. For the last two months of her life she had been unable to swallow any solid food; there was a stricture of the œsophagus, about two inches down; the tube was contracted to the size of a crow-quill. After death it was found that there was no ulceration, the disease consisting merely of a thickening of the mucous membrane and subjacent cellular tissue; there was no other symptom of disease in the neighbourhood; there was, however, a slight trace of malignant deposit in the glands of the mesentery.

Mr. Hale Thomson related a case in which a fatty tumour of the tongue was removed by excision. The swelling, which was simply adipose, was about the size of a walnut, and was situated about the middle of one side of the tongue.

Dr. Johnson inquired what was the shortest time in which any member had known purpura to prove fatal? He had lately seen a case in which the disease so terminated in four or five days, hæmorrhage occurring from every tissue in the body.

Dr. Addison had known a case to be fatal on the fourth day.

Saturday, January 12th.

IODIDE OF ARSENIC AND MERCURIALS IN LEPRA.—ACTION OF IODINE WHEN COMBINED WITH METALLIC BASES.—THE ENDERMIC USE OF MORPHIA.

Dr. A. T. Thomson stated that he had employed the iodide of arsenic in several cases of lepra and impetigo with very great success. A boy was at present in University College Hospital with the former affection; the eruption was spread over the entire surface of the body, and illustrated the disease in all its various stages. The affection had commenced three or four months before his admission, without any assignable cause, and had continued to increase in severity; the constitution also became cachectic. In this case emetics and purgatives were at first administered, and then small doses of iodide of arsenic and biniodide of mercury were exhibited internally, and the patient placed on milk diet. Local application of the common tar ointment was made to one part of the body, whilst a weak ointment of iodide of mercury was applied to the other parts; the latter remedy seemed to possess most influence. He (Dr. T.), however, thought neither of the local applications had been of much benefit. He mainly ascribed the improvement in the case, which was now very great, to the internal remedies. The boy's health had also much improved, and he had lately been able to lose small quantities of blood occasionally, a proceeding counter-indicated previously by the weakened state of the patient. Arsenic was well known to have a great influence over lepra, but it was slow in its action. The advantage of its combination with iodine consisted in the fact of its introduction into the system being facilitated and very much accelerated, the iodine acting on the absorbent, the arsenic on the cutaneous system. In the case related, he began with tenth-of-a-grain doses three times a day, and increased them to a quarter of a grain; in some cases he had never been able to get

beyond two-thirds of a grain, as symptoms of poisoning came on, and the medicine, consequently, required to be given in diminished doses. In the case of the boy, no ill effects had followed the medicine; his gums were now slightly tender: he had been three weeks in the hospital.

Mr. HORNE had found the local application of the tincture of iodine of much service in very obstinate cases of lepra. He had also used the tincture of lytta, with occasional advantage, in the same manner.

Mr. SMITH inquired if Dr. Thomson had ever treated a case with the iodide of arsenic alone.

Dr. WILLIAMS should be very sceptical in giving credit to any medicine in curing lepra on the evidence of a single case. No disease was more liable than the one in question to improve under even passive treatment, but none more likely to return, after a time, with increased severity. He had seen this occur in many instances when the disease had been treated by local and general remedies. Of all the medicines, however, which had been employed in lepra, he gave the preference to arsenic, which he had seen extensively used by M. Biett. There was something curious in the action of iodine when combined with other medicines; its own efficacy was not only augmented, but it also increased the effect of the metallic bases with which it was combined. In addition to its facilitating and accelerating their action, it also prevented that accumulation which was liable to take place with several powerful medicines, and which sometimes produced sudden and violent effects. This was the case with arsenic, iron, and mercury. From what he had seen of the iodide of iron, he should say that the iodine influenced the diffusion of the iron through the system. This was the case also with mercury when combined with iodine. No preparations, however, were more likely to get into disrepute than those of the character under discussion, in consequence of their being so extensively liable to decomposition, medicines with which they were incompatible being frequently mixed with them.

Dr. REID inquired if Dr. THOMSON had ever treated lepra with the biniodide of mercury alone? He (Dr. R.) had employed the old liquour oxymurias with the decoction of dalcamara with good success.

Dr. BROOKE inquired what was the greatest length of time during which the iodide of arsenic had been given in any one case?

Dr. THOMSON had employed the tincture of iodine as a local application to lepra in several cases without the least benefit. He had treated the disease with iodide of arsenic alone; the results were not so satisfactory as when this preparation was combined with mercurials. He had little reliance on any topical applications; he had never found any of them of much service. The case he had first related was one of many treated on the same plan, some of which had remained well to the present time, a period of many months. The iodide of arsenic was not liable to decomposition so readily as the iodide of iron. The former preparation could be kept for a long period in the form of pills, particularly if it had been sublimed. He had noticed something like an accumulation of this medicine in the system; for instance, he had not been able to detect the iodine in the secretions until it had been used several days; whereas it could be detected where iodide of iron was used in a few hours. He had at one time, generally treated lepra in the manner pursued by Dr. Reid, but he found that although the disease was materially improved, yet a long course of medicine was required, and the results were not generally satisfactory. In a case of carcinomatous depositions over the entire surface (LANCET, vol. ii., 36-37, p. 605), he had given the iodide of arsenic with the best success for three months: no inconvenience attended its use. He had not found that abstraction of blood was more useful in cases where arsenic was employed, than in those instances in which other preparations were administered. He generally placed his patients, when taking iodide of arsenic, under milk diet, but he gave no precautions as to the times at which it should be taken, or as to the full or empty state of the stomach.

Mr. SNOW inquired of Dr. Williams whether he should recommend the iodide of iron in cases where a salt of iron simply was indicated; or if he should confine its use, as was generally done, to cases in which iodine seemed to be required in addition to the tonic effects of the iron.

Dr. WILLIAMS replied that the iodide of iron would be more indicated when both the medicines entering into its composition were required. He should, however, have no hesitation in giving it in every case where steel was indicated. He had found, from some experience, that the preparation in question was more readily borne than most of the preparations of steel. If, however, there was one class of cases in which it exerted more beneficial influence than in others, he should say it was those cases, occurring in both sexes, in which there was a deficiency of the red particles of the blood in the system. In several of these cases, in which iron alone could not be borne, he had given the iodide with good success.

The conversation on this subject ending here,

Dr. THOMSON inquired the experience of members with respect to the endermic method of using morphia; and also, whether, after its application to a blistered surface for several successive days, they had noticed a papular eruption, which quickly became

vesicular, proceeding from the edges of the blister and gradually spreading over the entire surface. He had used the muriate of morphia frequently in phthisis; he sprinkled one or two grains of the morphia, mixed with a few grains of white sugar, over a blistered surface of about the size of a dollar, night and morning. The cough, in cases of phthisis, was materially relieved, and the sputa became of a better character. The same treatment pursued in the worst cases of neuralgia, had also been successful. He had found the occurrence of the eruption, which was not eczema, pretty constant after the use of the remedy for eight or ten days; the occurrence of the eruption affected the beneficial influence of the medicine.

The subject of embalming, as lately practised by M. Gannal, was now brought forward, for the particulars respecting which we refer to the Report of the Medico-Chirurgical Society. We may state, however, in addition, that Dr. Thomson said that he had examined the shavings of wood which had been placed in the bandages of two mummies, and had found that they contained a large quantity of tannic acid, a substance which is known to have a great preservative power.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, January 8th, 1839.

Dr. BRIGHT, President.

A CASE OF CARDITIS.

By THOMAS SALTER, *Esq., F.L.S., Poole.*

THE author begins by observing upon the extreme rarity of cases of genuine carditis, or inflammation of the muscular substance of the heart, a fact of which the older pathologists, who confounded this affection with pericarditis, do not seem to have been aware. "It does not appear," says the author, "that Corvisart or Laennec ever saw an unmixed case of carditis; nor does Andral give a single instance of the disease." The clearest case of the disease that has ever, in his opinion, been published, is that related by Mr. Stanley, in the 7th volume of the Society's Transactions; and even that presented unequivocal evidence of the co-existence of inflammation of the pericardium. The patient, whose case forms the subject of the present narrative, came under the author's observation eight days before he died, at which time he complained of uneasy sensations in the region of the stomach, and under the sternum, increased by exertion. On applying the ear to the region of the heart, nothing abnormal was discovered. The patient stated that he had first observed the symptoms six weeks before, whilst walking; that the pain was then at the lower part of the chest, inclining to the left side, and that, although it did not continue long, it was remarkably severe. A week afterwards he had a similar attack, following exertion. The attacks then became more frequent, and even lifting the arm sometimes gave rise to them. When the author saw him, he was sitting up in bed, being unable to lie down, owing to the great distress in his breathing. He indicated the middle of the sternum as the seat of the pain, which was not lancinating, but of a dull heavy description. The treatment adopted, including venesection, counter-irritation, and the administration of opium and calomel, failed to procure any alleviation of the symptoms, under which he sunk, retaining, however, his mind in a collected state during the entire period of his illness.*

On dissection, the vessels on the bag of the pericardium were distended with red blood, Its reflected layer, especially that covering the left ventricle, also evinced the existence of inflammation, which had proceeded to the greatest extent in the part of the membrane attached to the diaphragm, which presented ecchymosed spots, resembling hæmorrhagia. The substance of the heart was moderately firm; that forming the left ventricle had almost entirely lost its muscular colour, and pus could be scraped from its surfaces. In some parts there were small cavities in the muscular substance containing pus.

Dr. JOHNSON did not think carditis, or inflammation of the muscular structure of the heart, so rare a disease as the author of the paper appeared to suppose. In the numerous cases of hypertrophy of the organ, which succeeded acute pericarditis, or translations of acute rheumatism from the joints to the interior, where the parietes were thickened, red, and dense, had we not reason to believe that the morbid process was *carditis*, more or less acute, subacute, or chronic? In respect to the case under consideration, he (Dr. J.) thought it was a misnomer. It appeared to him to be one of suppuration, or rather deposition of purulent matter in different parts of the muscular structure of the heart, and not of common carditis. Such depôts in the parietes of the heart were undoubtedly of rare occurrence.

Dr. ELLIOTSON had seen a case of pericarditis occurring as a metastasis of articular rheumatism, in which the heart appeared to be remarkably enlarged. On examination after death, the heart was found of the

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* In Mr. Stanley's case, alluded to above, so entire was the absence of all symptoms referrible to the *heart*, that it was only by dissection ascertained that the patient did not die of disease of the *brain*.—*Sec. Med. Chir. Society.*

natural size, but there was found a deposition of lymph in the pericardium, by which the heart was elevated to a considerable distance from the diaphragm.

Mr. PALMER had witnessed a case of carditis, several years ago, in St. George's Hospital, under the care of Dr. Pearson. The patient was a young woman possessed of great personal beauty. The most remarkable circumstance of the case was the absence of any leading symptom by which the exact nature of her complaint might be understood, or any marked suffering of the patient; beyond a slight irregularity of the pulse, and a tendency to dyspnœa, which obliged her habitually to observe the semi-recumbent position, there seemed to be no serious interruption to the general health, and none whatever of the spirits; yet under these circumstances she died suddenly from a violent attack of dyspnœa. On examination, a small abscess was discovered in the parietes of the heart, containing about half a drachm of well-formed pus, with some surrounding softening of the muscle. Mr. Palmer did not recollect whether there was any attendant pericarditis.

Dr. KINGSTON remembered a case in St. George's Hospital which resembled that of Mr. Palmer, in the absence of all symptoms referrible to the heart, though a circumscribed portion of the left ventricle had assumed, through nearly its whole thickness, a light cream-colour, and so great a friability as to break down into a paste upon slight pressure. This, as well as a suppurative inflammation of the lungs, appeared to be consecutive on a fracture and suppuration of the neck of the thigh bone.

NOTICES OF THE EFFECTS OF LEAD ON THE SYSTEM.

By JAMES ALDERSON, *M.A., M.D., Physician to the Hull General Infirmary, &c.*

Among the numerous cases which have fallen under the author's care in the Infirmary of colic produced by lead, two have occurred with paralysis of the nerves of vision, which complication the author has nowhere seen described. These he has treated successfully upon the principle upon which Dr. Pemberton founded his recommendation to apply splints to the arms in cases of contraction of the flexor muscles from the same cause; namely, to relieve the continued tension of the extensor muscles, and thus to enable them to recover their suspended power. Reasoning analogically, the author supposed that, by removing the stimulus of light altogether from the eye, the power of vision would be regained, in which expectation he was not disappointed. He related two cases in which the loss of sight was added to the paralytic symptoms, and which were, in his opinion, relieved by the exclusion of light from the eyes. The treatment in each of these cases consisted, besides, in the administration of a drachm of Epsom salts, two minims of tincture of opium, and one ounce of infusion of roses, every four hours; ten grains of Dover's powder at bedtime, and the use of a stimulating liniment to the neck and spine, with a generous diet, and afterwards quinine. The author then offers some observations on the treatment of the more common effects of lead, and highly extols the remedial effects of croton oil in the more severe cases, in overcoming the torpor of the bowels. The paper concludes with the narrative of some cases of paralysis, from the unsuspected absorption of lead in consequence of drinking rainwater kept in leaden cisterns; and a note is appended to the paper by Mr. Pearsall, an able chemist, by whom a careful examination was made of the water, in the course of which he makes some important observations on the cautions necessary to be observed in drawing conclusions from such investigations, showing that the quantity of lead may, and does vary materially, with the nature of the substances mechanically present in the water, the whole being sometimes suddenly removed by agitation, as by a shower of rain, causing turbidness in the water.

Dr. JOHNSON considered the theory of Dr. Alderson, if true, to be very curious and important. Upon such doctrine he might cure deafness from paralysis of the auditory nerve, by long continued abstraction from sound; loss of appetite, by keeping the patient from food for days, or weeks, and so on, in various losses of function in different organs. His own opinion was, that the removal of the patient out of the sphere of the original cause of paralysis in the hands, and loss of vision, together with proper remedies and diet, contributed more to the recovery of sight, than the bandages on the eyes.

Mr. PALMER considered that the two points of interest in Dr. Alderson's communication were first, the production of paralysis of a nerve of sense from the poison of lead,—a very rare occurrence; and, secondly, the cure of the paralysis by an ingenious adaptation of Dr. Pemberton's method of treatment. He did not, however, consider that the evidence adduced was sufficient to establish either of these points. The paralysis from lead, as was well known, affected chiefly the *motive* power of muscles, and was accompanied, in most cases, with atrophy and degeneration of the muscular tissue, so as to induce the belief that the paralysis depended on the change of structure in the muscles, rather than on any affection of the nerves. There was, consequently, a want of analogy between the cases; or, in other words, between an affection consisting of mere loss of *power* depending on change of structure in the muscle, and loss of *sensibility* in an organ of sense depending on some affection of the brain or

the nerves. If we were to admit that the epilepsy which accompanied these cases proceeded from the lead, and the amaurosis from the epilepsy, or from the cerebral disturbance consequent thereupon, there would still be no analogy between the two cases, nor anything to justify the similarity of treatment. Regarding the treatment pursued, he thought, that as we were left to infer that the attendant muscular paralysis got well equally with the loss of sight, and at the same time, there seemed no reason for attributing the cure of one symptom more than of the other, to the bandages that were employed.

Dr. ADDISON concurred in the observations made by Dr. Johnson. He saw no parallel between the splints on the hands and the bandages over the eyes, in the cases in question. He doubted very much whether the application of splints to the hands, in cases of this kind, deserved so much credit as it obtained. The abstraction from the exciting causes of the paralysis, the attention to diet, and the regulation of the bowels, improved the general health, and Nature did the remainder of the work.

ON A NEW PROCESS OF EMBALMING, AND ON A NEW MODE OF INJECTING BODIES FOR DISSECTION, BY WHICH PUTREFACTION WILL BE PREVENTED.

The SECRETARY read a communication from Mr. George Smith, of John-street, Oxford-street, detailing some curious experiments made by M. Gannal during his late visit to this country, to show the preservative qualities of a fluid which he believed entirely prevented the ordinary effects of putrefaction in animal bodies after death. His communication was accompanied by several specimens of birds which had been subjected to the process, and which were found, at the end of more than two months, in a very extraordinary state of preservation; the flesh being perfectly soft and elastic, and not the slightest smell or taint discoverable, although no care had been taken to empty the crop of half-digested food, nor the intestines of feculent matter, nor had the birds been kept otherwise than freely exposed to the air of a common room with a fire in it.

Mr. GREGORY SMITH stated that at the end of last October, an application was made to him, on the part of M. Gannal, to allow him the opportunity of embalming a body, and also to test, by way of experiment, the virtues of a fluid for the preservation of animal bodies for the purposes of dissection. From the high scientific character of M. Gannal Mr. Smith at once assented; and on that gentleman's arrival in London, Mr. Smith procured for him the body of a man aged thirty-three, who had died of epilepsy on the 5th of November. On the 9th M. Gannal expressed his satisfaction with the appearance of the body, which was muscular and well proportioned, although the abdomen had already evinced some appearance of putrefaction, and there were some green marks also about the neck. M. Gannal's mode of procedure consisted in cutting down upon the left common carotid artery, and having introduced a common injecting pipe in a direction towards the heart, and applied a ligature upon the upper part of the vessel, in injecting, slowly, about four or five pints of fluid, no part of which was allowed to escape; a ligature was placed upon the vessel, and the incision brought together by suture. In about half an hour a great change came over the body; parts which had been previously soft and relaxed became firm and hard; the whole body resembled wax in appearance, and was nearly as firm. This part of the process being completed the body remained exposed to the air, and was seen by many persons. No perceptible change took place in the following three days, excepting that the green appearance of the abdomen and neck gradually disappeared. On the 12th of November M. Gannal attended and was well satisfied with the condition of the body, and in the presence of three or four persons, proceeded to complete the process of embalming, which consisted in placing linen in contact with the different parts of the body, moistened with spirit of rosemary, a single layer of oiled silk, and afterwards a similar layer of very thin sheet-lead; the whole of these things with the body, were afterwards surrounded carefully with many turns of bandages, employing altogether about two hundred yards. A small bottle containing the name of the individual embalmed, the circumstances under which it was done, and the names of those persons present at the embalming, was placed between the legs. The body was now entirely surrounded by bandages excepting the face and hands, which were purposely left exposed for observation. A transverse section of the cornea of each eye was made, to allow of the escape of the humours, and a glass eye was then introduced beneath the eyelids; a small portion of lint wet with oil of cloves was put into each nostril; the face and hands were lastly covered with a very thin coat of melted wax, applied with a small fitch-brush. The body, after two or three days, was soldered down in a leaden coffin, with two glass windows corresponding to the face and hands; a small quantity of powdered quick lime having been placed under the pillow upon which the head rested. On the same day, the 12th of November, M. Gannal injected the body of a middling-sized ass, with some of his preservative fluid, for the purposes of dissection. The animal had been killed only a few hours before by prussic acid, and the limbs were yet firmly contracted. About six or seven pints of fluid were injected from the carotid towards the heart, and left in the

vessels. The ass remained in the dissecting room untouched about three weeks, the temperature of the room in the day being about 65°. At this time, as there was a strong effluvium arising evidently from putrefaction, it was determined to remove the skin and ascertain the condition of the animal. It was found that the fluid had only injected its anterior half, which was in a good state of preservation; the other half was going rapidly into decomposition. The body of the man that has been embalmed, a part of the ass, and a number of birds, prepared by M. Gannal on his late visit, are now at the Theatre of Anatomy in Little Windmill-street, where Mr. Smith invited the Fellows of the Society to view them. There were two distinct fluids for embalming and injecting the body for dissection, neither of which could be substituted for the other.

Dr. MERRIMAN stated that he had seen the birds a week or two since, and that no particular precaution was taken in respect to temperature, but, on the contrary, they were kept hanging in a room with a fire in it.

Dr. KINGSTON said that he was sorry Dr. Macartney, of Dublin, had not imparted to the profession his mode of preserving bodies for dissection, or for the satisfaction of friends of the deceased. When Dr. Kingston attended his lectures, Dr. Macartney used often to demonstrate on subjects which he said had been preserved for many months, yet which retained a nearly natural colour and consistence, and were free from unpleasant odour. Dr. Macartney had in his museum the head and face of a man so well preserved that, though they were somewhat attenuated by drying, any one who had known the person during his life might have recognised his features.*

UNIVERSITY COLLEGE.

FIRST APPEARANCE OF DR. COPLAND.

DR. COPLAND made his first appearance as lecturer in place of Dr. Elliotson, on Monday evening last. As it was strongly rumoured abroad that he would meet with great opposition from the late Professor's party, considerable anxiety was manifested on the subject, and as early as seven o'clock, an hour before the time of lecture, there was already a good number of students in the theatre. By eight o'clock the theatre was literally crammed in every part, and there could not be less than four hundred persons present. From the approbation or disapprobation expressed when a leader of either party at the former meetings made his appearance, we were at first led to believe that the numbers in favour of the new lecturer and those against him, were pretty evenly balanced; this, however, as will be found in the sequel, was not the case. A few minutes before eight o'clock a voice from the top benches called out "Three cheers for Elliotson," but the proposition was met with a mingled storm of cheers and hisses.

Dr. Davis at this moment entering the theatre, the sounds were changed to one of general applause. The appearance of Dr. Copland, who walked in alone, none of the professors but Dr. Davis being present, was the signal for a general burst of acclamation, which, on subsiding, was met, on the Doctor's attempting to speak, by stamping upon the floor, all of which proceeded from the upper benches, on either side of the lecturer, and appeared to be raised by about a dozen persons on each side; this annoyance was persevered in for about twenty minutes, amid general cries of "Shame, shame, turn them out," Dr. Copland, several times attempting to address the audience in vain. Every now and then a cry was raised, but feebly responded to, of "Dr. Elliotson," which was met by a general clapping of hands and cheers for Dr. Copland, and cries of "No Elliotson; no humbug; no magnetism." The two little knots of persons who had so industriously kept up the riot, had ensconced themselves so snugly that they could not be detected by the angry assembly, for no sooner was attention directed to one side than the other immediately commenced the process of stamping, and thus the preconcerted game was carried on.

Dr. Sharpey now entered the theatre, and advanced to the table amid great applause; for a minute or two he could not obtain a hearing, but the anger of the assembly against the noisy minority became so extreme, that a number of students rose from their seats, and were proceeding towards the spot, when silence immediately was obtained, and Dr. Sharpey then said, "Gentlemen, on a single moment's reflection, it must be obvious to all who are friends of the institution, order, and the respectability of the profession, that this tumult cannot be allowed to go on, and unless it be quickly discontinued it will lead to the close of the class entirely." Here a voice exclaimed, "We'll have none but Elliotson," and some uproar was attempted to be made, but was immediately put down. "The meanest coward," continued Dr. Sharpey, "can hiss and express his disapprobation by uproar, when he dare not show his face (looking towards the part whence the tumult proceeded). In the late meetings every one had an opportunity of expressing his feelings; with those meetings I never interfer-

* The process employed by Dr. Macartney has been frequently communicated to the profession; it differs but little from that which was *subsequently* proposed by M. Gannal.—ED. L.

ed, but that affair is now terminated. In the temporary difficulty in which we have been placed, we have brought before you Dr. Copland, a distinguished writer and practitioner, it is your interest to give him a hearing. (Great cheers.) All who are friends to order, and wish to profit by the instruction he will give, will maintain silence." (Great cheers, and here and there a cry of "We want Elliotson.") The stamping now again commenced, amidst universal cries of "Shame." Dr. Copland proceeded to read his lecture; we had obtained a seat very near to him, but we could only occasionally take down a sentence. He did not, however, appear to us in any way to touch upon the late occurrences, but gave some general introductory remarks on the practice of medicine.

When Dr. Copland said that he hoped, by the indulgence and attention of the class, to finish the course without departing much from the plan pursued by Dr. Elliotson, there was great applause, and on an attempt being made to produce tumult, Mr. Taylor rose and said, that the disturbance was created by strangers, by individuals who had no right whatever to be in that theatre. Mr. Vance, a student, called out, in corroboration of Mr. Taylor's statement, that he had just been grossly assaulted by some strange men, who refused to give their names. (Cries of "Shame.") After this the lecture proceeded in tolerable quiet, excepting the occasional stamping of a solitary individual, or the slamming of a door; and, on one occasion, an attempt from a small party on the top bench, in the centre of which was Mr. Wood, to cause a disturbance; this was immediately, however, stopped by a student from that quarter saying, that if Dr. Sharpey would come up there, there would be no noise. With the exception of these interruptions, and the sounds of scuffling in the lobby, where, we are informed, there was a general fight between the intruders and a number of students, all was pretty still, and Dr. Copland concluded amid tremendous applause by saying, "Gentlemen, I return you my sincere thanks for the attention which you have paid me during the latter part of my lecture. It was to have been expected that there would, from generous minds, have been a demonstration in favour of their late teacher; but I hope at the same time that the indulgence you have shown during the latter part of this evening will be extended to my future lectures. All that it is in my power to communicate shall be imparted to you, and if there be any points respecting which there may be doubts in any gentleman's mind, I shall be always happy at this, and at other times, to communicate additional information."

In justice to several students we are happy to say that some of those who were leaders of the Elliotson party in the late meetings, were among the most attentive of Dr. Copland's audience.

On Tuesday evening Dr. Copland met with no kind of opposition in the delivery of his lecture. The statement made by Mr. Taylor, that the riot on the previous evening was caused by strangers, was we believe perfectly true.

PLAGUE AND QUARANTINE.

REPLY OF DR. BOWRING TO MR. NORRINGTON.

To the Editor of THE LANCET.

SIR:—I rejoice exceedingly that the controversy respecting plague and quarantine is beginning to excite attention. The paramount interests of truth will be advanced by the discussion. Yet I could have wished that your correspondent, Mr. Norrington, had read my pamphlet. Is it too much to expect that a critic should turn over the pages he criticises? Ought he to satisfy himself with an analysis, when the original work is so accessible? See, for instance, into what strange mistakes Mr. N. is led. As an example of the extraordinary *credulity* of the functionaries of lazarets (p. 11), I mentioned that "*the director of a quarantine on the Danube* told me he had discovered that when the Turks wanted to destroy the Austrians they inserted matter from the buboes of the plague behind the ears of the pigs intended for the Semlin market;" and this—this laughable fancy of an ignorant Austrian—Mr. N. assumes to be a "fact," and quotes it as evidence of the belief of the Mahomedans in the contagiousness of plague. Throughout the whole of my pamphlet I have drawn a distinction,—recognised by all travellers, and by the common phraseology of the Franks,—between *Levantines* (the Christian population of the East), and *Mussulmans*. I have stated that, with rare exceptions, the Mussulmans are unbelievers in the contagion of plague, but that the Levantines are believers; the Levantines, the most superstitious, the most timid of all the oriental races; those only between whom and Europeans intimate relations exist. To their statements most of the stories connected with plague are to be traced; they are credulous in the highest degree, and their credulity is mightily fed by their cowardice, as is their cowardice by their credulity. Your correspondent quotes a diamond merchant of Cairo as believing in the contagion of plague. Possibly; but very few indeed of the diamond merchants of Cairo are Mussulmans; and if the individual referred to by Mr. N. was a Mahomedan, I assure you that both in his religion and in his belief he differs from the majority of his brethren.

The contrast I put forward was this: that the Levantines, believers in contagion,

do frequently abandon their dearest connections when attacked by plague,—the Mahomedans never. I am told that of more than 50,000 Mussulman families visited by plague in Egypt in 1834-5, no instance is known or reported of a plague patient being fled from. I contrasted the conduct of the Mussulmans in cases of diseases of undoubtedly contagious character, with their conduct when the plague is amongst them. The incredulity of the Mahomedan races on the subject of the plague has been for centuries the theme of travellers—the recognised FACT which it has been sought to explain by representing them as *fatalists*. I endeavoured to show that this theory of *fatalism* will not explain their conduct. Mr. N., in opposition to all testimony, says, "*Most certainly* the Mahomedans do believe the plague to be contagious; at least I never met with one who did not." Mr. N. will serve the interests of truth and science if he can upset, by substantial and circumstantial facts, my theory, and establish his own. He acknowledges the authority of Clot Bey—Clot Bey, who has himself treated such multitudes of plague cases. Now I have, under Clot Bey's own hand, the declaration of the almost universal disbelief in the contagiousness of plague among the Arabo-Egyptian races.

Against Mr. N.'s statement, which I fear does not consist with his *own* knowledge and experience, that "in every case of plague which occurred in Jerusalem, it could be traced from individual to individual," I venture to place the declarations of almost every physician of authority in the East, that the plague is not so traceable. I shall rejoice if Mr. N. will declare, on his own personal veracity, that he did himself, by inquiry and by inspection, ascertain the facts he so positively announces. It was my discovery of the exceeding worthlessness of the evidence under this very head that so strongly excited my attention; it was from cases where communication was *impossible*; it was from instances where I found that the stories of communication with infected persons and infected districts had no foundation at all,—that my mind was more particularly directed to the inquiry. I intreat Mr. N., therefore, to state how "the Greek family brought the plague from Smyrna." It will show us that plague was in Smyrna when they left; it will show us how they avoided the quarantine at Jaffa, or elsewhere; and he will help us in the investigation as to the period in which the plague must necessarily have been latent in the sufferers.

Now, it is somewhat strange that I heard a similar story in 1838, when a few cases occurred in one of the convents; not a shadow of evidence could be given that any inmate of the convent had had communication with a plague patient. The Christian Levantines invariably *assume* communication, because they assume contagion; and hence, unless the evidence is thoroughly sifted, everybody is exposed to be led astray.

I hope Mr. N. will favour us with any fact or facts, to show that plague has been imported or communicated in regions freed from its endemic or epidemic visitations; that he will mention, in detail, the particulars of *his own case*, which must be highly interesting, and precisely of that class of facts which all searchers after truth would desire to collect. I would also call his attention and the attention of your readers to Mr. Holroyd's pamphlet, on the abuses and inconsistencies of Quarantine Laws, which has just appeared, Mr. Holroyd being, which I am not, a medical authority, and entitled to all attention. I am, Sir, yours, obediently,

JOHN BOWRING.

1, Queen-square, West,
Jan. 16, 1839.

REVACCINATION
AT THE
FOUNDLING HOSPITAL.

PROPRIETY OF REVACCINATING PERIODICALLY.

To the Editor of THE LANCET.

SIR:—I have recently submitted 216 children at the Foundling Hospital to the test of revaccination; and as this subject is just now creating so much interest amongst the members of the profession, it may be useful to lay before your readers the result of my observations upon these cases. Of these two hundred and sixteen revaccinated, *eleven* went through the different stages of *regular* cow-pox, as if they had never had the disease, proving that in them the primary vaccine had lost its prophylactic power.

In *one hundred and twenty-two* a spurious form of cow-pox was developed, producing, in some, considerable local inflammation and constitutional disturbance. The irregular nature and progress of the spurious affection in all these cases proved that the constitution was still under the protective influence of the first vaccination.

In *eighty-three* no effect (more than slight irritation from the puncture) was produced, though many of these were revaccinated twice; thus affording negative evidence of the undiminished preservative power of the primary vaccine.

I was led, in the first instance, to recommend a general revaccination of the children, from observing that the arms of many of those who had recently arrived from the country (of the age of five years), presented very imperfect cicatrices; but I think that

the following short observations will go far to prove that the condition of the cicatrix does not furnish any criterion whereby to estimate the success or efficacy of the early vaccination; and I feel justified in concluding that the intensity of the local inflammation produced by the vaccine virus, bears no relation to its subsequent protective virtue.

Of the *eleven* cases revaccinated, in which all the steps of natural cow-pox were regularly developed, eight presented *perfectly-formed cicatrices* (some having four, none less than two); *two* presented *no trace* of a cicatrix; and *one* had an imperfect scar. The youngest of the eleven was aged five years, the oldest aged thirteen years. I selected three arms, in neither of which could any trace of a cicatrix be discovered (though the children are all carefully vaccinated when infants), and I revaccinated them *twice* with the greatest care from a fine arm, without *any result* more than a slight degree of inflammation in one out of the three cases. Being desirous of employing only recent lymph from the infant's arm (for the greater security of the children revaccinated), I had no extensive opportunity of testing the lymph of those cases of successful revaccination, and the few experiments which I did make were inconclusive.

I will conclude this brief notice by earnestly calling the attention of the profession to the necessity of enforcing the practice of periodical revaccination. The period of time during which the vaccine may be said to preserve its prophylactic power does not seem to be agreed upon; and in the absence of any positive rules for our guidance, it is surely better that we should be on the safe side, and as we do see cases where we must believe that the regular cow-pox has been produced twice within the period of five years, we should consider this the maximum term of years over which the protective power of vaccine may be said to extend, and revaccinate, accordingly, every *fifth year*. I am, Sir, your obedient servant,

WILLIAM B. HUTCHINSON.

14, Guilford-street,
Jan. 12, 1839.

REDUCTION OF ARSENIURETTED HYDROGEN.

To the Editor of THE LANCET.

SIR:—Having been absent from town during the last fortnight, I was only yesterday aware that a letter signed "Candidus" had appeared in THE LANCET of Dec. 22, in which the author states what he calls "objections" to my mode of reducing arsenious acid, or arseniuretted hydrogen, for I allow the latter expression to be the correct one. If "Candidus" will do me the favour to reperuse my letter, he will find that I did not describe Mr. Marsh's apparatus as consisting of *a bottle and a tube*; on the contrary, I am well aware that he employed a bent tube, or syphon, for *generating* the gas, but the manner of its *escape* was through a straight tube, or "jet." My suggested improvement consists in allowing the gas to *escape through a tube bent downwards*, in order that when ignited it may burn within the tube, and deposit the metallic crust upon its walls. As regards the *quantity* of arsenious acid employed, I am aware that "a distinct crust" may be obtained from a much smaller quantity than five drops, but it is the *entire coating* of the end of the tube, and not merely "a distinct crust," that may be produced in the manner I described.

As to the risk of explosion from the admixture of atmospheric air with the hydrogen, it is only necessary to wait until the gas comes over briskly before it is inflamed, and then all danger of explosion is prevented. I have not found, as "Candidus" supposes, that the arsenic is *gone* before the gas can be inflamed, for pure hydrogen being lighter than arseniuretted hydrogen, *the former escapes first*, a fact of which any one may convince himself by watching the experiment. The risk of the glass flying in pieces is a trivial objection, for were it to do so the fractured extremity would still exhibit a crust, and the result be equally satisfactory. I may state, however, that after repeated trials I have not found this accident to occur.

I have only to observe in conclusion, that I readily admit Mr. Marsh's syphon to be well adapted for *generating* the gas, but as regards the mode of its *escape*, I would propose substituting a *bent tube* in the place of *a jet and stop-cock*, simply adapting the tube to the syphon by means of a cock. A common phial, with a bent tube, will, however, answer the purpose extremely well, and may always be easily obtained. I have the honour to be, Sir, your obedient servant,

G. A. F. WILKS, M.D.

5, Hart-street, Bloomsbury,
Jan. 12, 1839.

TREATMENT OF PAUPERS BY GUARDIANS.

To the Editor of THE LANCET.

SIR:—I beg to forward to you, and authenticate the following facts, which I think worth the attention of the profession. I shall make no further comment than that, as I consider any description of poor-law an evil, and the necessity that exists for one a disgrace to a community of Christians, the law ought to be as little objectionable as possible, and its operation conducted in a

just, liberal, and gentlemanly manner. I am, Sir, your obedient servant,

HENRY MEYMOTT.

Ludlow, 11th Jan., 1839.

I was requested, as one of the medical officers of the Ludlow Union, to visit a pauper, and report whether or not he was able to work. I found the poor fellow in a very debilitated state, and wrote a certificate to the Board of Guardians to the effect that I considered him totally incapable of work, and that it was my deliberate opinion, that he continued incapable from not being able to procure a sufficient quantity of food. He was only allowed 2*s.* a week, and but for a little that his wife earned, he and his family must have been famished. In consequence of my certificate he was allowed 2lbs. of mutton besides the 2*s.*, but as this was only granted for one week, I took advantage of my privilege, and ordered him 3 lbs. of mutton for the next week, filling up a printed form furnished me by the Board, and headed "Medical Order for Extra-Diet." Well, sir, I received the order back again, with this inscription:—"Jan. 7, 1839.—It is the unanimous opinion that Mr. Meymott should not have made this order, the man not being ill.—Charles Walcot, Chairman." So that the case stands thus: I certify that the man is ill, and incapable of work, provide him with medicine, and order him extra-diet; the Board of Guardians unanimously condemn my conduct, and declare that the man is not ill.

THE LATE THOMAS KING, ESQ.

WE have this week the melancholy duty of recording the decease of a worthy and talented member of our profession, Mr. Thomas King, who has been long advantageously known in London as a teacher of anatomy and surgery.

Mr. King was a native of Norwich, and at an early age evinced so great a predilection for the profession of a surgeon, that he was sent forthwith to study at the School of Medicine in Paris, where he competed successfully with his fellow-students, and at the first concours became member of the Ecole Pratique. He subsequently filled the offices of externe and interne at several of the Parisian hospitals, and while at the Hôtel Dieu was considered Dupuytren's favourite pupil. The Baron treated him with more courtesy and favour than any other interne, requiring his assistance at every important operation, placing private patients under his care, and evincing a particular friendship towards him by frequent recommendation. At this period Mr. King suffered from derangement of the digestive organs, and occasional rheumatism, caused by his zealous and unremitting labours in the dissecting-rooms, and particularly in the *salle des morts* of the Hôtel Dieu, where he gained fresh laurels by perfecting the records of the hospital cases, but judging from the event, probably laid the foundation of disorder which caused his premature end, and accordingly, like Grainger and Bennett, he has fallen a victim to his zealous pursuit of anatomical knowledge. The dead-house of the Hôtel Dieu is a dismal stone vault, in which anatomical inspection of the dead is performed with an accuracy unpractised in the English hospitals; and Mr. King was daily seen, shortly after six o'clock, even in the winter months, his feet protected from the pavement by sabots or clogs, his fingers punctured and wounded in dissections, enveloped in pieces of linen, and himself exposed for hours to a tainted atmosphere, biting cold, and misery scarcely conceivable by those who have not witnessed it. Few of those who have had charge of the dead-house pass through life without feeling the direful effects of the duties which have been required of them.

At the time Mr. Bennett established a School of Anatomy and Surgery in Paris, Mr. King assisted him, and subsequently lectured on the same subjects at La Pitié. His lectures were attended by foreigners, particularly Germans, as well as by many of his own countrymen.

In the year 1828 Mr. King came to London, and presented himself at the College of Surgeons for examination, which, after some hesitation, was granted, fully convinced that well-earned laurels and devotion to the profession would ensure success, that merit must be acknowledged, and that patronage was of secondary importance; but he soon discovered his error, for on offering himself on several occasions for public situations, it was made painfully evident to him that merit alone would not suffice. His advance in the profession was not so rapid as he anticipated, and the English system of appointing medical officers being so different from the French, had led him to express himself very warmly on the necessity of medical reform, of the adoption of the concours in the election of all public medical officers.

In 1831 Mr. King was appointed surgeon to the French Embassy, a high mark of distinction, when we consider that Prince Talleyrand was the representative of France at this period; and we have reason to believe that Mr. King was a favourite with the Prince, who took great pleasure in discussing medical opinions. The immediate cause of Mr. King's death appears to have been slow inflammation of the membranes of the brain and spinal marrow, causing sleeplessness, great irritability, and general wasting. From his doubting the nature of his disease the treatment pursued was very inert and not worth mention.

Mr. King has many claims on the profession as a contributor to medical science. His "Dissertation on the Ligature of the Innominata and Subclavian Arteries," which was the subject chosen for his thesis, on taking his degree in Paris, in 1828, shows correct anatomical and surgical views. The same subject formed the substance of a paper read before the Westminster Medical Society, and was reported in the medical weekly journals of the time, viz., February, 1831. His work "Lithotomy and Lithotrity Compared," published in 1832, is distinguished by a remarkably accurate description of the anatomy of the parts concerned in these operations, and was most favourably reviewed by the different periodicals. His plan and instrument for removing uterine polypi are simple and efficacious, the full description of which appeared in THE LANCET of Jan. 26, 1833.

Acupuncture, as used successfully by him, in cases of hydrocele and ascites, and his modification of the operation for cataract, have been already tested by experience, and are decided improvements on the old methods of treating those diseases. An account of these operations will be found in the "British Annals of Medicine," No. 1, 1837, and LANCET for the past year.

The first part of the article "Amputation," in Mr. Costello's "Cyclopædia of Practical Surgery," is marked by industry and talent of a superior order.

His lecture delivered at the reopening of the Blenheim-street School, and published in 1834, displays more intimate acquaintance with general and comparative anatomy and chemistry than is usually considered essential to the mere anatomical teacher.

Mr. King's lectures were marked by accurate and close reasoning, and a strictly logical order. His manner of delivery was energetic and impressive, and he possessed the power of fixing the attention of his class in an eminent degree. His friendly and open conduct endeared him to all his pupils and colleagues.

Mr. King was a strenuous advocate for the *concours*, as was remarkably evinced by his answer to Mr. Warburton on his examination before the Committee of the House of Commons on Medical Education. The question was—"Does the *concours* act as a powerful stimulus to medical students?"—"It acts as a tremendous stimulus," was Mr. King's energetic reply.

The medical profession has lost in Mr. King a highly distinguished and most honourable member, and the writer of this a sincere and much valued friend.

SCHEME OF EXTENSION
OF THE
ROYAL COLLEGE OF SURGEONS IN IRELAND.

THE Dublin College of Surgeons has recently received from its "Committee of Correspondence," a Report of proceedings in which the Committee make the following statements and propositions. The Committee was appointed in September last, to communicate with various persons and associations in the medical profession, in order to enlarge the influence of the College, and increase the number of its friends and supporters, or, in the words of the Committee "to identify its interests with those of the general mass of the medical practitioners of Ireland," whether members or not of that institution. The Committee state, that amongst the letters which they have received are many containing marked evidences of dissatisfaction at the "oppressions and hardships" to which the Irish country practitioners "have been subjected in consequence either of the defective state of medical law, or the tyranny of local authorities." Steps have been taken for the redress of these grievances, of which the following instances are given:—

"Medical witnesses were compelled to attend courts of quarter sessions without any means of payment being provided. A representation has been made to Government, and a promise obtained that this defect of the law shall be considered with a view to its removal.

"In another case, it having been proved that presenting sessions were in the habit of cutting down orders for remuneration which had been given by coroners to medical witnesses for their services at inquests, the matter has been submitted to Sergeant Greene, and his opinion obtained to the effect, that it is illegal for these sessions to modify or cut down such orders, although they may absolutely reject or confirm them. Hereby it is hoped that future annoyance on this point will be spared to medical practitioners."

The Committee state that several medical associations, especially in the South of Ireland,—being desirous of a more general union to promote their objects, and believing that the College is willing to, and capable of, furnishing "a head and rallying point to the whole body of Irish medical practitioners,"—have (in those instances in which the members are not Fellows of the College) expressed "a desire to be received into the College as licentiates." The Committee having looked at this proposition "in every shape and light which occurred to them," and especially in connection with "the present posture of medical affairs," are convinced that it should be accepted, in order that "a general union of the profession

under such a vigorous, intelligent, and liberal head as the College would furnish," may be accomplished, and the College itself be thus enabled to occupy the "hitherto unattained position of a national medical body," which should regulate medical education, "watch over the interests and conduct of its members, and obtain for the profession in Ireland equal political rank and importance with that of the law, or the ecclesiastical corporations." Until, "say the Committee, something of this sort be provided, and medical knowledge be thereby made to constitute a necessary portion of the great social machine, it never can be considered as anything but an empirical art. Medical men may turn their talents and attainments to collateral branches of knowledge; or may hunt after cures, and dignify their pursuits by the name of science; but, until they are consulted as a body upon matters regarding the public health, they can never be considered in any other light than as the menial servants of individuals, to be used when sickness makes occasion for their services, and to be cast off and neglected directly upon the return of health."

The Committee observe that this suggestion may, to some persons, appear of a startling character, and imply as it were, the cheap parting with an advantage by the present members of the College which has been purchased by much labour and expense. Your Committee, however, conceive that nothing can be more certain than that the elevation in rank and importance of any collective body, must of necessity confer additional importance upon every member already belonging to it, and that thus the privilege would be made vastly more valuable by sharing it with their professional brethren. By others it may be objected, that the union proposed would not be sufficiently extensive to constitute (by the mere force of numbers) this College as the head of the medical body. Your Committee reply, that they would gladly see it effected by a still more comprehensive union; and to some of them it would be matter of high gratification, if the other medical corporations of Ireland could see things in the same light.

To effect this union, the Committee propose "That a special act of grace be passed by the College, to last three or six months, during which period, and no longer, it may be competent for the Court of Censors to grant licenses, *upon a formal examination*, to *such* graduates of other Colleges as may be *recommended by a committee to be nominated by the College;* and that the Committee shall be instructed to recommend no person for such distinction who has not been a *bonâ fide* medical practitioner, of *five years'* standing, whose moral and professional character they have not ascertained to be respectable, or who keeps a shop for the sale of medicine or drugs."

With regard to the fee payable by such persons on their admission, the Committee "have come to the conclusion that, upon taking into account the heavy expenses incurred by them in procuring degrees and diplomas, and the not very wealthy condition of the profession in this country, *ten guineas* might probably be considered a fair sum, with reference to the advantages sought and procured."

"Having made this statement, the Committee entreat for it the calm and deliberate attention of the College."

CORRESPONDENTS.

A.B. The Poor-Law Amendment Act does not contain any clause which will save a person who practises as an apothecary without being legally qualified by the Act of 1815, from the penalties of a successful prosecution under the latter statute.

W.R. is right. The canons ecclesiastical (the canons of 1603) by which the clergy of this country are all bound, although they were not ratified by Parliament (having passed both Houses of Convocation), expressly forbid the exercise of any secular occupation by the clergymen of the United Church of England and Ireland, with the following exceptions: the farming of land (glebe or otherwise) not exceeding eighty acres; but that only with the express written permission of the bishop; the editing of any literary periodical, newspaper, or magazine; the calling of a schoolmaster or tutor. At the time of instituting the London College of Physicians one of the regulations then passed declared that if a Fellow entered into Holy Orders the Fellowship, *ipso facto*, became vacant. We do not publish the letter of *W. R.*, because no comments could add to the effect of the *verbatim* copy of the letter criticised.

The communications of *Dr. Lynch*, *Mr. Obré*, *Mr. Denham*, and *Mr. Stevens*, shall appear next week.

Communications have been received from *Mr. J. C. Hall*, *Mr. Snow*, *Mr. W. Roberts*, *Mr. Wadsworth*, *Mr. Dendy*, *Dr. Hancock*, *Mr. Teevan*, *Mr. Baird*, and *A Reader of* THE LANCET.

We have already answered, more than fifty times, the question put by *C. D.* No; it will not.

Will *Chirurgus* privately authenticate the circumstance relative to the stoves?

Corrigenda.—We are requested by Mr. Burgess to correct the following errors, which, upon reference, we find to have occurred in Dr. Blundell's manuscript copy of his letter, p. 534. For "Mr. S. Burgess," read Mr. A. Burgess; line 9, for "two," read ten; line 17, for "as much as," read as nearly as possible; line 18, for "of," read for; line 22, for "acid," read and; line 23, for "weight," read weights; line 25, for "draught," read draughts.